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TRANSACTIONS

OF THE

AMERICAN CLIMATOLOGICAL

AND

CLINICAL ASSOCIATION

FOR THE YEAR 1917

VOLUME XXXIII

"The object of this Association shall be the Clinical Study of Disease, especially of the Respiratory and Circulatory Organs, and of Climatology and Hydrology."—Constitution.

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1917

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NOTICE.

The Association assumes no responsibility for the statements and opinions expressed in the papers read at its meetings.

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LIST OF OFFICERS.

Presidents.

Name	Year
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V. Y. Bowditch	1889
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S. E. Solly	1895
J. B. Walker	1896
E. Fletcher Ingals	1897
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Beverly Robinson	1899
Abraham Jacobi	1900
Robert H. Babcock	1901
Samuel A. Fisk	1902
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James C. Wilson	1904
W. F. R. Phillips	1905
E. L. Shurly	1906
Thomas Darlington	1907
Thomas D. Coleman	1908
Charles E. Quimby	1909
Edward R. Baldwin	1910
John Winters Brannan	1911
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Charles L. Minor	1913
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Judson Daland	1917

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A. H. Smith, E. O. Otis.....1893
I. Hull Platt, E. L. Trudeau.....1894
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S. A. Fisk, John C. Munro.....1897
Beverly Robinson, C. F. McGahan.....1898
James A. Hart, R. C. Newton.....1899
R. H. Babcock, J. W. Brannan.....1900
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1888. Abbot, Griffith E., 722, Main Street, Worcester, Mass.
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1898. Sunderland, Septimus, 11, Cavendish Place, Cavendish Square, W., London.
O. M. Tyndale, J. Hilgard, Richards Block, Lincoln, Nebraska.
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ELECTED

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1914. Alsever, William D., 727, East Willow Street, Syracuse, New York.
1899. Anders, James M., 1605, Walnut Street, Philadelphia.
1890. Anderson, B. P., 1231, N. Cascade Avenue, Colorado Springs, Col.
1900. Arnold, Horace D., 520, Commonwealth Avenue, Boston.

1893. Babcock, R. H., 74, East Elm Street, Chicago.
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ELECTED

1898. Baldwin, Edward R., Saranac Lake, N.Y.

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1897. Blackader, Alexander D., 236, Mountain Street, Montreal, Canada.

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1897. Bonney, S. G., Stedman Building, Denver, Col.

1895. Boardman, W. S., 63, Mt. Vernon Street, Boston.

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1901. Bracken, Henry Martyn, 1010, Fourth Street, S.E., Minneapolis, Minn.

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1894. Bridge, Norman, Temple Auditorium, Los Angeles, Cal.

1903. Brown, Lawrason, 104, Main Street, Saranac Lake, N.Y.

1903. Brown, Philip King, 516, Sutter Street, San Francisco, Cal.

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1907. Browning, Charles C., Merritt Building 8 and Broadway, Los Angeles, Cal.

1909. Bushnell, George E., U.S.A., c/o Surgeon-General, War Dept., Washington, D.C.

1886. Butler, G. R., 226, Gates Avenue, Brooklyn.

1914. Byers, John Roddick, Ste. Agathe des Monts, P.Q., Canada.

1896. Campbell, W. A., 424, N. Nevada Ave., Colorado Springs, Col.

1910. Carrington, P. M., U.S. P.H. Service, San Diego, Cal.

1906. Claytor, Thomas A., 1826, R. Street, N.W., Washington, D.C.

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ELECTED

1894. Coleman, Thomas D., 505, Greene St., Augusta, Ga.
1901. Collins, Charles Farnham, 50, West 55th Street, New York City.
1889. Coolidge, A., 613, Beacon Street, Boston.
1917. Craig, Frank A., 244, South 21st Street, Philadelphia.
1910. Crane, Bayard T., Rutland, Mass.
1907. Da Costa, John C., jun., 264, South 15th Street, Philadelphia.
1892. Daland, Judson, 317, South 18th Street, Philadelphia.
1890. Darlington, Thomas, 27, Washington Square North, New York City.
1907. Darnall, Wm. Edgar, 1704, Pacific Ave., Atlantic City.
1897. Davis, N. S., 7, West Madison Street, Chicago.
1910. Dixon, Samuel G., Harrisburg, Penn.
1905. Dunn, William LeRoy, Asheville, N.C.

1897. Edson, Carroll E., McPhee Building, Denver, Col.
1915. Elliott, John Barnwell, jun., 608, Maison Blanche, New Orleans, La.
1903. Elliott, J. H., 11, Spadina Road, Toronto, Canada.

1912. Farrand, President Livingston, Boulder, Colorado.
1910. Floyd, Cleaveland, 246, Marlborough Street, Boston, Mass.
1917. Forster, Alexius M., Colorado Springs, Colo.
1911. Fulton, Frank Taylor, 36, Prospect Street, Providence, R.I.

1896. Gardiner, C. Fox, 818, North Cascade Avenue, Colorado Springs, Col.
1892. Gibson, William M., 260, Genesee Street, Utica, N.Y.
1915. Goodman, Edward H., 248, South 21st Street, Philadelphia.
1909. Griffin, Walter Alden, Sharon, Mass.

1907. Hall, J. N., 452, Metropolitan Building, Denver, Col.
1912. Hamman, Louis, 714, Park Avenue, Baltimore.

ELECTED

1893. Hance, I. H., Lakewood, N.J.

1891. Hart, James A., P.O. Box 144, Geneva, N.Y.

1912. Hastings, Thomas W., 172, East 71st Street, New York.

1912. Hatfield, Charles J., 2008, Walnut Street, Philadelphia.

1914. Hawes, John B., 2nd, 29, Gloucester Street, Boston, Mass.

1896. Heffron, John L., 608, East Genessee Street, Syracuse, N.Y.

1912. Hewlett, A. Walter, Leland Stanford University Medical School, Sacramento, and Webster St., San Francisco, Cal.

1893. Hinsdale, Guy, Hot Springs, Virginia.

1902. Hoagland, Henry W., 818, North Nevada Avenue, Colorado Springs, Col.

1914. Holden, G. Walter, Agnes Memorial Sanatorium, Denver, Colorado.

O. M. Ingals, E. Fletcher, Monroe Buildings, 104, So. Michigan Boulevard, Chicago.

1889. Jacobi, A., 19, East 47th Street, New York.

1888. Jayne, W. A., 408, Metropolitan Building, Denver, Col.

1901. Jennings, Charles Godwin, 457, Jefferson Street, Detroit, Michigan.

1915. Johnston, Collins H., 87, Monroe Ave., Grand Rapids, Michigan.

1907. Kinghorn, Hugh M., 14, Church Street, Saranac Lake, N.Y.

1899. Klebs, Arnold C., c/o — M. Forbes and Co., Sears Bldg., Boston.

1917. Klotz, Walter C., Barlow Sanatorium, Los Angeles, Calif.

1910. Landis, H. R. M., 11, South 21st Street, Philadelphia.

1914. Lee, Roger I., Harvard University, Cambridge, Mass.

ELECTED

1914. Lichty, John A., 4634, Fifth Avenue, Pittsburg, Pa.
1917. Lloyd, John, Iola Sanatorium, Rochester, N. Y.
1909. Locke, Edwin Allen, 311, Beacon Street, Boston, Mass.
1913. Lord, Frederick T., 305, Beacon Street, Boston.
1904. Lowman, John H., 1807, Prospect Ave., S.E., Cleveland, O.
1907. Lyman, David Russell, Wallingford, Conn.

1906. Manges, Morris, 72, East 79th Street, New York.
1902. Marvel, Philip, 1616, Pacific Avenue, Atlantic City, N.J.
1917. Matson, Ralph C., 1021, Corbett Building, Portland, Oregon.
1917. Matson, Ray W., Corbett Bldg., Portland, Oregon.
1887. Mays, Thomas J., 1829, Spruce Street, Philadelphia.
1917. Meader, Charles N., 304, McPhee Bldg., Denver.
1915. Mercur, Wm. H., 5244, Fifth Ave., Pittsburg, Pa.
1905. Miller, James Alexander, 379, Park Ave., New York.
1909. Miner, Charles H., 115, South Franklin Street, Wilkesbarre, Pa.
1899. Minor, Charles L., Asheville, N.C.
1917. Montgomery, Charles M., 2210, Locust Street, Philadelphia, Pa.
1909. Morgan, James Dudley, 919, Fifteenth Street, N.W., Washington, D.C.

1895. Newton, R. C., 42, Church Street, Montclair, N.J.
1907. Nichols, Estes, 188, State Street, Portland, Maine.
1907. Nichols, John B., 1321, Rhode Island Ave., N.W., Washington, D.C.
1917. Niles, Walter L., 56, East 77th Street, New York.
1912. Norris, George W., 1530, Locust Street, Philadelphia.

1888. Otis, E. O., 381, Beacon Street, Boston.

1913. Parfitt, C. D., Gravenhurst, Ontario, Canada.
1912. Paterson, Robert Childs, Saranac Lake, New York.

ELECTED

1906. Perkins, Jay, 125, Waterman Street, Providence, R.I.
1895. Phillips, W. F. R., School of Medicine, Charleston, S.C.
1917. Piersol, George Morris, 1913, Spruce Street, Philadelphia.
1887. Platt, Walter B., 802, Cathedral Street, Baltimore.
1917. Porter, George D., 162, Crescent Road, Toronto, Canada.
1902. Pottenger, F. M., 1100, Title Insurance Building, Los Angeles, Cal.
1905. Pratt, Joseph H., 317, Marlborough Street, Boston.
1917. Price, J. Woods, Saranac Lake, N.Y.
1914. Probst, Charles O., 185, State Street, Columbus, Ohio.
1905. Pryor, John H., 26, Linwood Ave., Buffalo, N.Y.

1891. Quimby, Charles E., 278, West 86th St., New York.

1885. Rice, C. C., 123, East 19th Street, New York.
1901. Richardson, Charles W., 1317, Connecticut Avenue, Washington, D.C.
1904. Richer, A. J., Ste. Agathe des Monts, Province of Quebec, Canada.
1917. Ringer, Paul H., Asheville, N.C.
1890. Robinson, W. D., 2012, Mt. Vernon St., Philadelphia.
1902. Rochester, Delancey, 469, Franklin Street, Buffalo.
1890. Rogers, E. J. A., 222, Colfax Avenue, Denver, Col.
1917. Russell, Nelson G., 469, Franklin Street, Buffalo, N.Y.

1905. Schauffler, William Gray, 400, Madison Ave., Lakewood, N.J.
1901. Sewall, Henry, 1360, Vine Street, Denver, Col.
1915. Shattuck, George Cheever, 205, Beacon Street, Boston.
1910. Shurly, B. R., 32, Adams Avenue W., Detroit, Mich.
1887. Smith, Frank Fremont, Bar Harbor, Maine.
1917. Smith, Wm. H., 10, Gloucester Street, Boston.
1911. Steiner, Walter R., 4, Trinity Street, Hartford, Conn.
1909. Stevens, Martin L., Asheville, N.C.

ELECTED

1904. Stone, Arthur K., 44, Fairfield St., Boston, Mass.
1915. Stone, Willard J., Colton Building, Toledo, Ohio.
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1901. Swan, Will Howard, 1440, North Nevada Avenue, Colorado Springs, Col.

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1917. Waters, Bertram H., 40, East 41st Street, New York.
1917. Webb, Gerald B., Colorado Springs, Colo.
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1911. Williams, Linsly R., Albany, New York.
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o. m. Wilson, James C., 1509, Walnut Street, Philadelphia.
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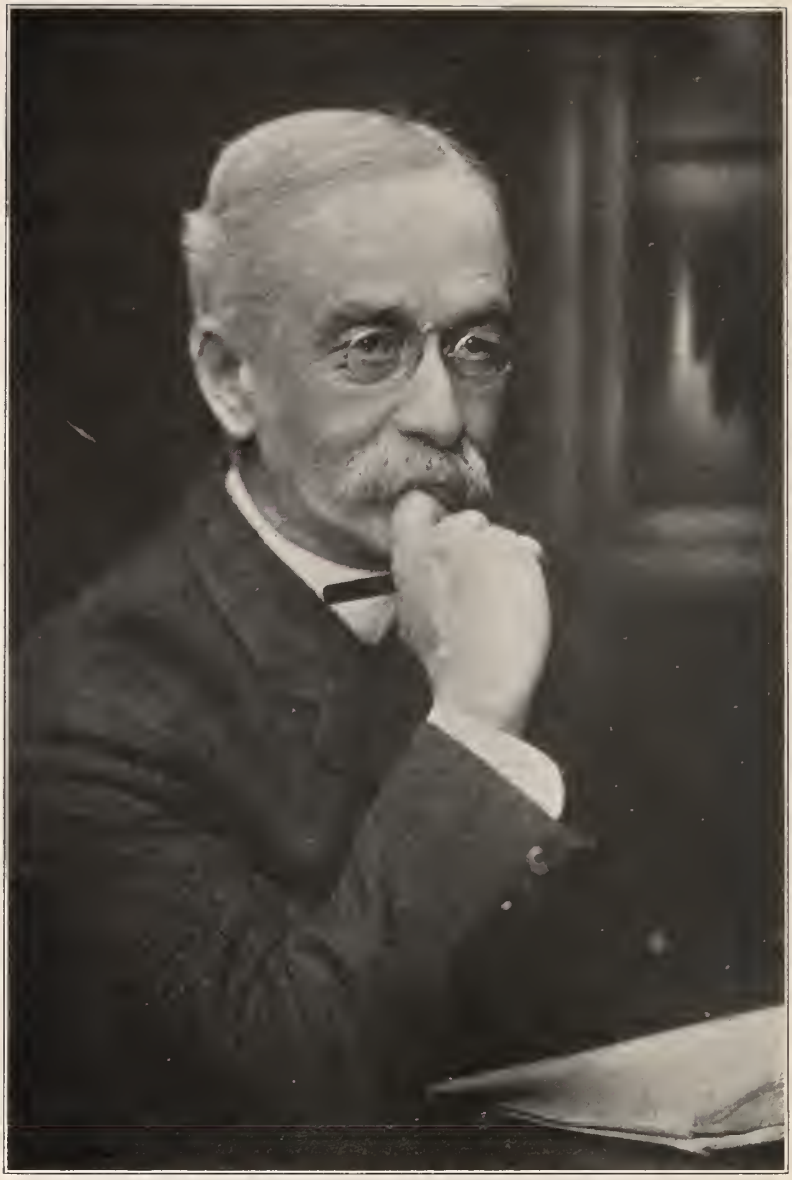
1913. Wood, Nathaniel K., 520, Beacon Street, Boston.



EDWARD WILLIAM SCHAUFFLER, A.B., M.D.

Born September 11th, 1839. Died October 29th, 1916.

MEMORIAL.

EDWARD WILLIAM SCHAUFFLER, A.B., M.D.

Dr. Edward W. Schauffler, a charter member of this Association, was born on September 11, 1839, in Vienna, Austria, where his father, Rev. William Gottlieb Schauffler, D.D., one of the pioneer American missionaries at Constantinople, Turkey, was spending a year. His mother, Mary Reynolds Schauffler, came from Longmeadow, Mass., of Puritan stock. His early boyhood years were spent in Constantinople, and at the age of twelve, having even then acquired a knowledge of several Oriental languages, including Turkish, he acted as interpreter to the commanding General of the British Army in the Crimea. Later he served as an attaché in the American Embassy at Constantinople, and then came to this country to complete his education at Williams College. He left college to study medicine, at first under a preceptor, and later at the College of Physicians and Surgeons, New York. This work was interrupted at the outbreak of the Civil War, by his joining the 127th New York Volunteers as First Lieutenant. He served through the war, coming out as Captain. As a member of the staff of General Schimmelpfenig, he was the first Federal officer to enter Charleston when it was taken by the Northern armies. At the close of the war, he finished his medical education, graduating at the College of Physicians and Surgeons, New York, in 1868.

He served as house physician at Bellevue Hospital, New York, and at the conclusion of that service he took up practice in Kansas City, Mo., then a town of about 25,000 inhabitants. He continued in active practice in Kansas City until his death from angina pectoris on October 29, 1916, after an illness of only a few days.

To quote from the *Medical Herald* of Kansas City: "He was probably the first physician to light on the ecru hills of Kansas City, who could write the Master of Arts degree after his name. Thus in refinement and culture, Dr. Schauffler was a stranger in a new land, a diamond among the rough, a classic in the midst of the unsettled; but he loved work with an uplift. He was active in medical organization leading to the establishing of the College of Physicians and Surgeons, later to become the Kansas City Medical College. To the time of the dissolution of the College, Dr. Schauffler was the President of the College Association. His untiring energy in the teaching of young men had much to do in sowing the seed of medical education that has developed the medical proficiency of our South and West."

Dr. Schauffler was active in most of the movements for the betterment of Kansas City, his especial interest being in health conditions. Largely through his efforts, open-air schools were built. For the last eight years he had been a member of the Board of Directors of the State Tuberculosis Sanitarium at Mount Vernon. He led the advocates of the construction of the County Tuberculosis Sanitarium at Leeds. He was professor of physiology, and later of the practice of medicine, in the Kansas City Medical College, until that school was absorbed by the Kansas State University. He filled this same chair in the Kansas State University for a number of years. He was a former President of the Jackson County Medical Association and of the Associated Charities, Kansas City, and a former member of the State Board of Health. He was a member of the Loyal Legion, of the American Medical Association, and of the Missouri Medical Association, of which he was also at one time President.

Dr. Schauffler was selected by the American editor of Ziemssen's Cyclopedia of Medicine as one of its translators, and his translation of Liebermeister's great and classic article on typhoid fever, which appeared in the first volume in 1874, was notably accurate and most creditable. His reputation as a Christian scholar and gentleman, as an upright and fearless citizen, and

HERBERT MAXON KING.

Born August 11th, 1864. Died 1917.

as an efficient and progressive practitioner of medicine, gained him renown throughout the whole section of the country where he spent forty-eight years of his life in doing good to others. His membership in our Association always gave him particular pleasure, and long after he was placed on the honorary list, he enjoyed hearing of its annual meetings, the programmes of which, with notes, were regularly sent to him.

He was a member of the Westport Avenue Presbyterian Church, in the work of which he took a very active part. He is survived by a widow, three sons and a married daughter.

W. G. SCHAUFFLER.

HERBERT MAXON KING,

1864–1917.

THE ranks of the tuberculosis workers have been sadly depleted since the beginning of the great war—Roemer and Cornet in Germany, Landouzy in France, Trudeau and now King in America. In the present ferment and turmoil of war, in the face of deaths by thousands, the loss of a few workers, it might seem, could be passed over in silence. To those, however, who have been engaged in the struggle against tuberculosis, the loss of leaders is ever deeply felt.

Born at Adams, N. Y., August 11, 1864, Herbert Maxon King studied first at Dartmouth College and later (1887) obtained his degree in medicine from the New York University Medical College. After studying in Paris and serving for a time in the Marine Hospital Service, he began practice in Grand Rapids and specialized in nose and throat work until 1902 when he was called to take charge of the Loomis Sanatorium.

Here his real life work began. He took charge of an institution that had been, to say the least, not well managed, and so reorganized and developed it that today it stands preëminent among the private sanatoriums for tuberculosis in the east. His task

was not easy, for he dealt largely with patients financially able to do as they chose, but his personal interest in them, his charm of manner and well recognized ability kept many for years in the sanatorium in order to remain in close touch with such a man.

In the department devoted to poorer patients, need arose for increased accommodations, which led King to study and devise the first shack to be used in the treatment of tuberculosis. The erection of this shack revolutionized in America the ideas of construction of sanatorium quarters. A little later he built a model infirmary for patients needing absolute or partial rest. Combined with this strict rest in bed when necessary, he followed for years the method of auto-inoculation, having his patients at first do forestry but later build roads. He published from time to time the results of his experience with this form of treatment and as an adjunct to it papers on the opsonic index. The food problem was diligently studied and dietitians put in charge of the table. His papers are valuable additions to the knowledge of this subject. An extensive study of the post-discharge history of the patients from the Loomis Sanatorium was made for he, like other sanatorium workers, had asked the question how worth while it was. His answer was a decided affirmative. This work led to the consideration of prognosis and it is interesting to note that his first and last papers dealt with this subject. While still in Michigan he was impressed with the resistance to tuberculosis of children with parental tuberculosis. He attributed this resistance at that time to the effects of heredity, clearly recognizing the results due probably, in the light of our present knowledge, to repeated small infections.

He was deeply interested in scientific work, as the fine Babbitt Memorial Laboratory of the Loomis Sanatorium bears testimony. His enthusiasm led to its erection.

King was a loyal and enthusiastic member of the "Climatological" and Clinical Association from his election in 1904 until his death. Failing health prevented his attendance at some of the more recent meetings.

A look at the man revealed his artistic temperament; his very dress betrayed it. His hobby was the study of church architecture and his knowledge of it profound. His greatest pleasure was a tour of the cathedrals of England and France and he felt bitterly the destruction of Rheims, for this (June, 1914) was the last foreign cathedral he ever visited.

Strong of purpose, intolerant of pettiness of action or deed, conscious of the faith that was in him, King has gone from us. Above his knowledge of tuberculosis, above his scientific attainments there remain to his friends the picture of a wonderful and charming personality and the memory of a true friend.

L. B.

CHRONOLOGICAL LIST OF PAPERS READ BEFORE MEDICAL SOCIETIES OR CONTRIBUTED TO THE MEDICAL PRESS BY DOCTOR HERBERT MAXON KING.

A Contribution to the Study of the Etiology of Tuberculosis. Read at meeting of the Grand Rapids Academy of Medicine, May 28, 1894. Printed in the *Medical Record*, November 24, 1894.

Physical Diagnosis and Modern Therapeutics. Read at meeting of Michigan State Medical Society, 1896.

A Case of Renal Phthisis Complicating Pulmonary Tuberculosis. Read at meeting of Grand Rapids Academy of Medicine, March 9, 1896. Printed in the *New York Medical Journal*, July 11, 1896.

A Case of Tuberculosis Involving the Hip-joint, Lungs, Testicles, Inguinal Glands, and One or More of the Vertebræ. Read at meeting of the Michigan State Medical Society, May 14, 1897. Printed in the *Medical News*, October 2, 1897.

A Case of Genito-Urinary Tuberculosis: Necropsy. Read at meeting of Michigan State Medical Society, May 14, 1897. Printed in the *Medical News*, September 4, 1897.

A Clinical Study of the Action of the Nucleins in Tuberculosis. Printed in the *Medical News*, May 22, 1897.

Résumé of One Hundred Cases of Tuberculosis. Printed in the *Medical News*, December 3, 1898.

A Case of Arteriosclerosis: Necropsy. Printed in the *Medical News*, April 1, 1899.

The Prognosis of Pneumonia in its Relation to its Etiology. Read at meeting of the Michigan State Medical Society, May 4, 1899. Printed in the *Medical News*, July 8, 1899.

State Sanatoria for Consumptives in Michigan. Discussion on Tuberculosis at the 35th annual meeting of the Michigan State Medical Society in 1900. Printed in the *Medical News*, December 8, 1900.

An Interesting Case of Splenic Anemia. Read at meeting of the Grand Rapids Academy of Medicine, October 16, 1900. Printed in the *Medical News*, March 23, 1901.

Post-operative Non-septic Leucocytosis and Other Blood Conditions. Read at meeting of the Chicago Pathological Society, May 12, 1902. Printed in *The American Journal of the Medical Sciences*, September, 1902.

The Care of the Consumptive Poor, with Especial Reference to the Establishment of a Consumption Hospital in Connection with the County Farm. Read at the annual meeting of the Grand Rapids Charity Organization Society, June 13, 1902. Printed in *The Physician and Surgeon*, August, 1902.

On the Healing of Tuberculosis (Clinical Features). Read at meeting of the Laennec Society. (No date given.) Printed in the *Medical Record*, January 7, 1905.

Sanatorium Provision with Industrial Opportunities for Indigent Consumptives. (By Doctor Herbert Maxon King and Doctor Henry B. Neagle.) Prepared for the Symposium on the Sanatorium Treatment of Consumption at the first annual meeting of the National Association for the Study and Prevention of Tuberculosis, May 19, 1905. Printed in the *Medical News*, October 7, 1905.

Diet in Tuberculosis. Read at second annual meeting of the National Association for the Study and Prevention of Tuberculosis, May 16, 1906. Printed in the *Medical Record*, July 21, 1906.

The Scope and Value of the Sanatorium in the Anti-Tuberculosis Movement. Read at sixth annual conference of Sanitary Officers of the State of New York, October 24, 1906. Printed in the *Medical Record*, December 15, 1906.

Notes of Early Diagnosis of Pulmonary Tuberculosis. Read at meeting of the Third District Branch of the Medical Society of the State of New York, October 22, 1907. Printed in the *Medical Record*, December 14, 1907.

Local Anti-Tuberculosis Societies. Printed in the *Journal of the Outdoor Life*, March, 1908.

The Tuberculo-Opsonic Index in its Relation to the Temperature Curve in Active Tuberculosis, and its Value in Diagnosis in Suspected or Arrested Cases. Printed in the *Medical Record*, July 3, 1909.

On the Construction of an Efficient and Economic Diet in Tuberculosis. Read at fifth annual meeting of the National Association for the Study and Prevention of Tuberculosis, May 13, 1909. Printed in the *Medical Record*, October 16, 1909.

Vaccine Therapy in Tuberculosis in Institutional Practice. Read at a joint meeting of the American Otological Society, the American Laryngological Association and the American Climatological Association at the Eighth Congress of American Physicians and Surgeons, 1910.

Institutional Care of Tuberculosis. Printed in the *Journal of the Outdoor Life*, April, 1910.

Therapeutic Artificial Pneumothorax. (By Doctor Herbert Maxon King and Doctor Charles Wilson Mills.) Presented in abstract at the mid winter meeting of the American Sanatorium Association at the Montefiore Sanatorium, Bedford Hills, New York, December 7, 1912.

Section on "Tuberculosis" for Forchheimer's "Therapeusis of Internal Diseases," issued by D. Appleton & Company in 1913.

A Preliminary Study of the Value of Sanatorium Treatment. Read at the eighth annual meeting of the National Association for the Study and Prevention of Tuberculosis, May 30, 1912.

Exercise as a Therapeutic Measure in Pulmonary Tuberculosis. (By Doctor Charles W. Mills and Doctor Herbert Maxon King.) Read at the tenth annual meeting of the National Association for the Study and Prevention of Tuberculosis, May 7, 1914.

Contribution to the Study of Prognosis in Tuberculosis. Read at meeting of the Laennec Society, May 5, 1916. Printed in the *Bulletin of the Johns Hopkins Hospital*, November, 1916.

CONSTITUTION AND BYE-LAWS.*

CONSTITUTION.

Article I.—Name.

This Society shall be known as the American Climatological and Clinical Association.

Article II.—Object.

The object of this Association shall be the Clinical Study of *Disease, especially of the Respiratory and Circulatory Organs, and of Climatology and Hydrology.*

Article III.—Membership.

Section 1.—This Association shall consist of *active, corresponding, Emeritus,* and *honorary* members, the former not to exceed 150 and the latter not to exceed twenty (20).

Section 2.—Names of candidates for active membership, whose applications shall have been endorsed by *three* (3) active members, shall be sent to the Secretary at or before the annual meeting at the second business session of which they shall be read and then lie over until the next annual meeting, when such as are approved by the Council shall be balloted on. Three (3) black balls shall be sufficient to reject a candidate. The Council shall have power to nominate active members.

Candidates for membership shall, at the discretion of the Council, present a paper to the Association showing clinical study of merit.

Section 3.—The power of nominating honorary and corresponding members shall be vested in the Council. The election shall be conducted in the same manner as that for active mem-

* As amended May 30, 1917.

bers. Honorary members shall enjoy all the privileges, but shall not be allowed to hold any office or cast any vote.

Section 4.—Anyone who has been an active member of this Association for twenty (20) years in good standing, and has attained the age of sixty (60) years, may become an Emeritus member by expressing his desire to do so, in writing to the Council, and he shall thereafter be exempt from all dues and duties of the Association.

Section 5.—Any member of the Association absent from the meetings, in person or by contributed paper, for three (3) consecutive years, without sufficient cause, shall be dropped from the list of members by vote of the Council.

Article IV.—Officers.

Section 1.—The officers of this Association shall consist of a *President*, two *Vice-Presidents*, a *Secretary and Treasurer*, and a *Recorder*, who, with the Delegate to the Executive Committee of the Congress of American Physicians and Surgeons or his alternate and five other members, shall constitute the *Council* of the Association.

Section 2.—*Nominations.* The officers, including the Council, shall be nominated by a committee of five (5) members, which committee shall be nominated by the President at the first session of each annual meeting and shall report at the business meeting.

Section 3.—*Elections.* The election of officers shall take place at the business meeting. A majority of votes cast shall constitute an election.

Section 4.—The President, Vice-Presidents, Secretary and Treasurer, and Recorder shall enter upon their duties at the close of the annual meeting at which they are elected, and shall hold office until the close of the next annual meeting, or until their successors are elected.

Section 5.—Members of the Council, other than the President, Vice-Presidents, Secretary and Treasurer, shall hold office for five (5) years.

Section 6.—*Vacancies.* Any vacancy occurring among the officers of the Association during the year may be filled by the Council.

Article V.—Duties of Officers.

President and Vice-Presidents.

Section 1.—The President and Vice-Presidents shall discharge the duties usually devolving upon such officers. The President shall be *ex-officio* Chairman of the Council.

Secretary and Treasurer.

Section 2.—As Secretary, he shall attend and keep a record of all the meetings of the Association and of the Council, of which latter he shall be *ex-officio* Clerk. At each annual meeting he shall announce the names of all who have ceased to be members since the last report. He shall notify candidates of their election to membership. He shall send a preliminary notification of the annual meeting two (2) months previous thereto, and the programme for the annual meeting at least two (2) weeks previous to its assembly, to all the members of the Association. He shall also send notification of the meetings of the Council to the members thereof. At each annual meeting of the Association he shall read the minutes of the previous meeting and of all the meetings of the Council that have been held during the current year.

Section 3.—As Treasurer, he shall receive all moneys due, and pay all debts therewith. He shall render an account thereof at the annual meeting, at which time an auditing committee shall be appointed to report.

Recorder.

Section 4.—The Recorder shall secure the papers read and also proper notes of the discussions thereon and shall superintend, under the direction of the Council, the publication of the Transactions.

Article VI.—Council.

The Council shall meet as often as the interests of the Association may require.

Four (4) members shall constitute a quorum.

It shall have the management of the affairs of the Association, subject to the action of the Association at its annual meetings.

It shall consider the claims of candidates recommended to it for admission to membership.

It shall not have the power to make the Association liable for any debts exceeding in total one hundred dollars ($100), in the course of any one year, unless specially authorized by a vote of the Association.

It shall have the entire control of the publications of the Association, with the power to reject such papers or discussions as it may deem best.

It shall have power to nominate active members at the annual meeting.

The Council shall have power to invite any gentleman, not a member, to read a paper at the annual meeting, on any subject within the scope of the objects of this Association.

The Council shall determine questions by vote, or—if demanded—by ballot, the President having a casting vote.

The Council shall constitute a Board of Trial for all offences against the Constitution and Bye-laws, or for unbecoming conduct, and shall have the sole power of moving the expulsion of any member.

The President, or any two members, may call a meeting, notice of which will be transmitted to every member two (2) weeks previous to the meeting.

Article VII.—Papers.

Section 1.—The titles of all papers to be read at any annual meeting shall be forwarded to the Secretary not later than one (1) month before the first day of the meeting, in order to appear on the printed programme.

Section 2.—No paper shall be read before the Association which has already been printed or been read before another body.

Article VIII.—Quorum.

A quorum for business purposes shall be ten (10) members.

Article IX.—Amendments.

This Constitution may be amended by a four-fifths ($\frac{4}{5}$) vote of all the members present at an annual meeting, provided that notice of the proposed amendment has been printed in the notification of the meeting at which the vote is to be taken.

BYE-LAWS.

(1) Meetings of the Association shall be held annually.

(2) The time and place of the meetings shall be determined by the Council.

(3) The dues of active members shall be ten ($10) dollars on election and thereafter shall consist of an annual assessment not to exceed seven and a half ($7.50) dollars. Members in arrears shall not be entitled to vote. Those in arrears for two (2) years may be dropped from membership by recommendation of the Council.

(4) Order of business meeting.

First day:—

Calling the roll of members.
Minutes of previous meeting.
Secretary's and Treasurer's reports.
Recorder's report.
Report of Committee of Arrangements.
Appointment of auditing committee.
Appointment of nominating committee.
Report of Council on recommendations for membership.

Second day, Morning session:—

Reports of nominating committee and auditing committee.
Election of officers.
Election of members.
Nomination of candidates for membership.
Report of committee on health resorts.
Unfinished business.
Miscellaneous business.
Adjournment of business meeting.

SECRETARY AND TREASURER'S REPORT.

This association has finished its thirty-third year. We are returning to Lakewood where we met 21 years ago. Sixteen of those who met with us are no longer living but we will ever cherish their memory. We have also suffered losses during the past year; Drs. Whitcomb, Kyle and Casselberry among the active members and Dr. E. W. Schauffler, an honorary member, have passed away. This leaves us with a total membership of 129, one having resigned and one being transferred to the emeritus list.

The great war has also brought sorrow to the homes of some of us, our member Dr. Blackader and our honorary member Sir Frederick Stupart having lost their sons on the battlefields of France.

Among our members who are serving in the medical corps of the Canadian army are Dr. J. Roddick Byers, a captain, and Dr. J. H. Elliott, a captain, while another Canadian, Captain George D. Porter, is a candidate for election.

These medical officers are serving their king and country most faithfully in heartrending times. Many cases of exposure to noxious gases as well as wounds received in battle have come under their care.

Our honorary member, Surgeon-General Gorgas, is directing the medical corps of our army with his well-known ability. Another honorary member, Dr. E. L. Gros, for years well known in Paris with an important service in the American Hospital, is in charge of the selection of men for the aviation corps. Dr. George C. Shattuck who did so much work in Serbia with Dr. Strong, of Harvard, has returned to the work in France with the Harvard Unit. Dr., now Major, Roger Lee has gone with the Peter Bent Brigham Hospital Unit. He is chief of the Medical Section U. S. A., Army Base Hospital No. 5. Several of our members are in the Medical Reserve Corps, among them our president,* who holds a commission in the Navy and an ex-President, Dr. Anders, a captain in the Army. Others in the service include: Dr. Horace D. Arnold, a major on active duty at the headquarters of the Northeastern Department of the Army in Boston; Dr. George W. Norris, who sailed for France, as Major in the Pennsylvania Hospital Unit; Dr. W. G. Schauffler, who is on active duty as Surgeon-General of the New Jersey National Guard. Lieut.-Col. George E. Bushnell is on duty at the Surgeon-General's Office in Washington. Others on the reserve corps of the Army include: Dr. T. D. Coleman, Dr. J. A. Miller, Dr. P. K. Brown and Dr. J. C. Wilson. Major J. H. Pratt, Major Linsly Williams, Major E. H. Goodman, National Guard, Pa.; Major J. N. Hall, Major T. D. Coleman, Major W. A. Jayne, Major T. W. Hastings. Contract Surgeons: Drs. Otis, Claytor, Cleaveland Floyd, J. Gurney Taylor and Willard J. Stone, Charles M. Montgomery, H. R. M. Landis. Dr. George Morris Piersol, a candidate for election, is in the Officers' Training Camp at Fort Oglethorpe. We should be obliged if any names accidentally omitted would be supplied.

The war has also had the effect of delaying the appearance of our transactions; but we trust that any disappointment at its non-appearance thus far will be pardoned inasmuch as unforeseen conditions arose to delay the work.

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* Dr. Daland has since been promoted to Lieut. Commander.

FINANCIAL STATEMENT.

Balance in hand May 2, 1916	$ 451.86
Receipts from dues	1,034.80
Total	1,486.56
Expenses	949.90
Balance in hand May 20, 1917	$ 536.76

All bills which have been rendered to date have been paid. The increased cost of paper and printing no doubt will add to our expenses.

The following change in the by-laws has been recommended by the Council:

Resolved: That by-law No. 3 shall be changed by inserting after the word "shall" in the first line the words "be ten dollars on election and thereafter shall" so that the first sentence shall read: (3) The dues of active members shall be ten (10) dollars on election and thereafter shall consist of an annual assessment not to exceed seven and a half ($7.50) dollars.

The Secretary then read the minutes of the last meeting of the Council, held in Philadelphia on January 11, including the list of candidates for honorary and active membership, as approved by the Council for election at the present meeting.

REPORT OF THE RECORDER TO THE COUNCIL OF THE AMERICAN CLIMATOLOGICAL AND CLINICAL ASSOCIATION.

VOLUME 32, for the year 1916, remains unpublished. The first delay was caused by the long waits before a number of the authors of the papers returned the proofs sent them. In fact several have not yet been returned.

After this came the war difficulties in getting mail matter to the publishers in London. The corrections made to the proofs were so many and important that the printing and binding of

the uncorrected proofs surely would have been entirely unsatisfactory to the authors.

Photographs of Drs. Trudeau and Smith had to be made here after the proofs were returned.

A typewritten copy of the entire transaction has been made so that if the original should be lost reproduction could still be made.

I learn that any matter beyond a page letter is likely to be detained by the censors.

Business men sending invoices, mail three copies in one- or two-page sections a few days apart and have each registered.

Business men of large experience at present who ought to know have advised not to send the matter until after the censorship has been removed, which is expected now at an early date, since the United States is an ally of England.

The delay has been a matter of great regret to your recorder but to act otherwise than he has was in the judgment of our president, Dr. Daland, and of Dr. Jas. M. Anders, of our censors, with whom he has been in frequent conference, and in his own judgment, unjustifiable.

Your pleasure and direction is sought in the matter.

Respectfully submitted,

Wm. Duffield Robinson.

PHARMACISTS TO AID THE SURGEONS.

By a happy coincidence we received communications simultaneously from Dr. J. Madison Taylor, of Philadelphia, and from Dean Wulling, of the School of Pharmacy of the University of Minnesota, president of the American Pharmaceutical Association, urging the recognition of pharmacy in the army as a means of enlisting the coöperation of the pharmacists of higher attainments. Both of these were published in our issue of July 21. The organization of our army on a peace footing required

no recognition of pharmacy as a separate calling. Our medical supplies were purchased, packed in convenient receptacles, and ready for dispensing, which could be done by almost any intelligent layman. The comparatively small pharmaceutical skill required under the organization of the army in times of peace was easily acquired by the more intelligent enlisted men of the Medical Department. Now that we are at war, however, and our army has been suddenly increased to ten times its former size with prospects of still further enlargement, it seems highly desirable that the physicians in the Medical Corps should be relieved of the details of the medical supply service, and that this should be turned over to skilled pharmacists of high professional training. This could only be done successfully by creating a pharmaceutical corps with commission rank.

This proposal met with the approval of the Section in Pharmacology of the American Medical Association at the meeting held in New York, and has also been approved by the journal of the Association. In view of the acknowledged difficulties in obtaining the services of the required number of medical officers, it would seem that the physicans in the Medical Corps would be exercising an enlightened selfishness if they too would lend their aid to the movement begun by the American Pharmaceutical Association for the establishment of a commissioned pharmaceutical corps in the Medical Department. Not only would the pharmacists of a higher type, who might thus be induced to join the service, be useful in the purchase, preparation, and distribution of medical supplies, but they would be of great help in making clinical examinations of sputum, urine, etc., in doing certain kinds of bacteriological work, in examining foodstuffs and water as to potability, and in doing other chemical work which is now either neglected or performed by medical men who can be ill spared for purely medical fields of labor.

Doctor Taylor quite properly urges that the army and navy surgeon be given the expert assistance which could be rendered by such a pharmaceutical corps. In the armies of every first

class power, except Great Britain and Russia, the value of the pharmacist's service has long been recognized by commissioned rank. In the armies of Italy and France the chief pharmacist is a brigadier general; in the German army, before the war, he bore the rank of colonel, though his rank has probably been raised since that time. In the Japanese army the chief pharmacist was a lieutenant colonel, and in the Spanish army a colonel. Even in the British army there are a few pharmacists who have commissioned ranks, though there is no organized corps of commissioned pharmacists. It is to be noted that all those nations which maintain large standing armies have recognized the value of the services of the expert pharmacist by conferring commissions upon them. Surely the soldiers of the United States are entitled to have the services of men of the highest degree of skill in every line.

LOCALIZED ARTERIOSCLEROSIS.

By THOMAS D. COLEMAN, A.M., M.D.,

Professor of Medicine, Medical Department, University of Georgia, Augusta, Georgia.

Arteriosclerosis in its ultimate is the triumph of disease or age or floth; it is the surrender of the mortal. If the arteries could be prevented from becoming sclerotic, it is conceivable that death, except through violence or accident, might, as Metchnikoff has contended, be indefinitely deferred. Nothing that lives exists without nourishment, and the nourishing mechanism must continue working truly, in order that life may continue. As the arteries and arterioles lose their normal character and become sclerosed, their functional activity becomes impaired, nutrition interfered with and in consequence the whole body suffers.

Localized arteriosclerosis has for many years been recognized pathologically and clinically as occurring from childhood to old age; in the child from disease, in the aged from wear and tear. Generalized arteriosclerosis is found in the diseased and in the aged; universal arteriosclerosis may be regarded as a medical curiosity, or perhaps, more literally, never exists.

These varying degrees of arteriosclerosis bring into play physical and clinical problems which are of the utmost moment in the treatment of the disease and the prolongation of human life. If we could prevent the development of arteriosclerosis, or cure it after it had definitely developed, it is superfluous to proclaim that the length of days accredited to the patriarch Methuselah would at least pass without marvel; one might even be pardoned for wondering why he had not lived longer.

Concerning the effects of arteriosclerosis on the metabolism

of the different organs of the body, or the body as a whole, consideration cannot be given in detail at this time. Physiologists all agree that with a sound heart and circulatory mechanism, the requisite pabulum to the tissues is furnished through the positive pressure exerted by the heart, the elastic recoil of the arteries, the peripheral resistance offered by the capillaries, a slight suction action on the part of the veins and a vital or selective activity on the part of the cells lining the circulatory tract. Anything that affects the healthy or normal action of any of these will influence the metabolism of the organs supplied by them.

If these elements are all perfect and functioning normally, then blood pressure estimates may be accepted and certain deductions made therefrom, but here error creeps in, and it is to some of these errors that I wish to direct your attention.

Blood pressure is estimated in two ways: first, directly through the agency of a canula inserted into an artery of the animal studied, and usually never employed upon man, and, second, indirectly, through the medium of some physical device which may be employed upon man without injury to him. Both methods are subject to fundamental elements of error, but since the former do not concern us now, we may with profit endeavor to estimate the clinical and scientific value of the latter.

My viewpoint does not allow me to escape the question of the elements of error entering into the problem, *e. g.*, when I take a patient's blood pressure is that the real tension under which the tissue metabolism of that individual is usually carried on, is the reading affected by the presence of a pad of fat an inch or more thick over the artery under examination; or by muscular activity and mental perturbation on the part of the patients, at the time of examination? Then comes in the never-ending question of the personal equation, which not only includes delicacy of touch. education, but acuteness of mind as well.

But dismissing these elements of error that all must consider, we still have other problems that cannot be dismissed, if we

are to estimate the value of the factors which end in death, on the one hand, or result in length of days on the other. A sage has enunciated the axiom that if the finest thread were equally strong throughout, no power conceivable could break it, and similarly it may be assumed that if the arterial tree, as found in all mammals, were perfect throughout, localized hemorrhage would be impossible. Generally speaking, in proportion as arteriosclerosis develops and the arteries and arterioles lose their elasticity, you have rigid instead of elastic vessels and blood pressure increases.

We have adopted certain standards of pressure expressed in mm. of mercury, for healthy men according to age, and taking a hundred or a thousand men with relatively intact arteries these may be approximately correct; but if the arteries are locally sclerosed, if we are guided by our readings alone, we may be seriously misled. Osler has observed that in healthy young adults the blood pressure may be elevated ten to twenty mm. by physical exertion, and Daland has called attention to the fact that emotion or excitement may cause a similar rise. If the arteries are sound throughout the circuit, all well and good, but if they are not, if they are materially sclerosed, in the brain we will say, what sphygmomanometer will tell whether they will withstand the strain, and not rupture? An illustration of this simple principle of physics may be found in a city that has an old system of water pipes, and requires more pressure for its needs; a new reservoir is established at a higher level which affords the necessary additional pressure, but the additional pressure has to be used with caution and check valves employed, else the old pipes will give way under the strain; it is not feasible except in rude fashion to tell how much the old pipes will withstand because the rusting or decay is not uniform in each pipe. The analogy is not perfect, but the same principle holds in our arterial pipes. Decay and rust may cause obstruction to the onflow of the water and require greater pressure, in order that needs may be met, but instruments of precision cannot measure what each pipe will stand,

unless they can be adjusted to each pipe in the circuit, which is practically impossible. In the question of localized arteriosclerosis and blood pressure, we have other elements entering into consideration. In a former paper, I endeavored to show how it is necessary to risk the chance of rupture in order that the essential nourishment of special organs and the well-being of the body may continue, and pointed to the fact that it was in some cases not only unwise and unscientific to reduce blood pressure even though it were high, but that, on the contrary, it was actually necessary to increase it in order to prolong life. But the decision to do this should be arrived at not from the blood pressure reading alone, but from a close study of all the correlated symptoms. Among these may be mentioned, the hypertrophied heart, sclerosed arteries, polyuria, frequent urination, especially nocturnal, small amounts of albumen and hyaline casts in the urine, digestive disturbances, headache, fulness in the head, insomnia, restlessness, nervous apprehension, and at times even mental aberration.

Again, what would seem to be excessive blood pressure in some individuals is really necessary for them, even if we may not call it normal; likewise a relatively low blood pressure in others may not excite our apprehension, though it would if we only knew all the conditions. I have known patients to live fairly healthy and very active lives with blood pressures over two hundred mm. On the other hand within three months I have had a patient of sixty years of age, with a blood pressure of one hundred and sixty mm., to suffer from a stroke of apoplexy. I have a patient under observation now, a man over fifty, one of the hardest professional workers I have ever known, who has a slightly hypertrophied heart, sclerotic vessels in his retina, and (from his nervous phenomena) in his brain, I believe; polyuria and occasionally traces of albumen and hyaline casts in his urine; and yet his blood pressure has never been over one hundred and twenty-eight mm. We are too prone, I am convinced, to take a blood-pressure reading and console ourselves

with the thought, and our patient with the assurance, that since the blood pressure is only one hundred and fifty or one hundred and sixty all is well. I very much fear that it gives us a false sense of security. I have a number of cases among my records where the blood pressure was not appreciably increased, or perhaps I should say sustainedly increased, before the apoplectic stroke supervened but in every one of them one or more of the mental symptoms that I have mentioned were present. In conclusion what I desire most to emphasize with the hope that it may be of service to other clinicians is that we should not delude ourselves or mislead our students with the doctrine that a useful instrument of precision, the sphygmomanometer, is infallible. I have a contempt for an iconoclast, meaning thereby a man who destroys an image and has nothing to put in its place, and it is not my purpose to smash this clinical idol that claims so many worshippers.

It supplements but cannot cannot supplant the educated touch, and mature clinical judgment. These come only through years of apprenticeship.

DISCUSSION.

Dr. Elliott: Dr Coleman has said very little about specific instances of arteriosclerosis. I should like to ask him, or you. the Fellows of the Association, if any advice can be given me in a case I have had under observation foi six or seven years—man about 63 years of age whom I first saw on account of his inability to keep his left foot warm and a certain amount of cyanosis. The condition is due to localized arteriosclerosis of the vessels below the knee in the left leg. The circulation in the right leg is normal and the arteries are not appreciably thickened, but on the left the dorsalis padis is definitely thick, and the X-ray shows a calcareous artery from the knee down. He has not improved to any appreciable extent during six years' observation. He has to keep the leg wrapped in cotton for warmth and cannot let it hang down for any length of time because of the pain it causes. He goes about in a wheel chair. He is anxious to have the leg amputated, but the surgeons hesitate to amputate because of the possibility of non-union of the flap, as the arteries in

the thigh may be to some extent involved. It is a condition difficult to deal with, and I should like to know whether any of the Fellows of the Association can give me any advice concerning it. The man's pressure with a sphygmomanometer in the brachial artery is usually about 160. His general condition is good.

Dr. PERKINS: I saw some years ago a case of localized arteriosclerosis which I think is the most interesting I have ever seen. All the symptoms were related to the abdomen. I don't know the history fully further than that the patient went up to New Hampshire in the summer and returned in the fall worse than when he went up. When he came to me all I could make out on examination was some dulness extending down from the liver. I thought he had a cancer of the liver and referred him to a man who could take care of that condition. He died soon afterwards, and at the autopsy at first nothing was found. The heart, the arch of the aorta and the descending aorta above the diaphragm were all in perfect condition. At the point where the celia axis comes off there was a section one inch in length thickened and markedly calcarious, partially closing the lumen of the blood vessels. The mass felt attached to the liver was old fibrous tissue. Otherwise the organs were normal.

Dr. DALAND: I have been much impressed in recent years by the extraordinary importance of what may be called septic (?) endocarditis. In other words, there seems to be considerable evidence of the involvement of the arteries through septic emboli. It is very easy to understand that in a certain number of the well-marked cases we might have an explanation of what might be called a localized arteriosclerosis—so called from the fact a particular region was especially involved. I should like to ask Dr. Elliott about that specifically. In his case in all probability the process was further advanced in the left leg than elsewhere.

Dr. COLEMAN (closing): I have nothing more to add. This paper is in one sense a preliminary paper, as to how the subject has influenced my own thought. It does not begin to cover the whole field. I have not attempted to go into the question of the etiology, for that would have prolonged the paper to too great length.

As to Dr. Elliott's question in reference to his patient I feel that it would be useless to attempt to give any advice, because I know Dr. Elliott so well and I know he must have tried all the things that any of us who are well up in this subject would have tried. I don't know whether he has tried massage. Possibly that might help.

COMPROMISING WITH CONSUMPTIVES.

By JOHN B. HAWES, 2d,

BOSTON.

I HAVE written this paper because I feel that what has always been a difficult problem to me may be a source of difficulty to others, and that its frank discussion may lead to its solution. The question in brief is this: What is consumption? What does this word mean, both in diagnosis and treatment? Have we any definite, set, minimum standards or requirements that must be met before we can say that this or that patient is a consumptive and must be treated as such?

When I was in the medical school, some sixteen or seventeen years ago, the diagnosis of pulmonary tuberculosis rested largely on the finding of bacilli in the sputum; in their absence, the signs in the lungs had to be very plain and distinct, including dullness, bronchial breathing, râles, etc., before a positive diagnosis was considered. At present we seem to be requiring less and less on which to base a diagnosis. In Massachusetts, with the establishment of local tuberculosis dispensaries in every town or city of 10,000 inhabitants or over, this problem is becoming acute. Physicians are in charge of these clinics whose business it is to discover cases of consumption. The result is that many cases are diagnosed as such on what I personally believe to be insufficient evidence.

Some physicians still demand the presence of bacilli in the sputum before making a positive diagnosis. Others demand definite signs in the lungs before so doing; while still others attach more value to the history and constitutional signs and symptoms than to signs in the chest. The result is chaos, as far as any definite, fixed standards are concerned.

With the possible exception of syphilis, I know of no other subject of equal importance concerning the diagnosis and treatment of which there seems to be so much confusion and disagreement. In Massachusetts, for instance, one of our very best men, a strong believer in the value of percussion, has said that "interscapular dullness plus some constitutional signs" justifies a tentative diagnosis of tuberculosis in childhood. Another physician of equally high standing justified his diagnosis of tuberculosis in a certain child on the ground of exposure from a grandmother and because the child lived in a notoriously crowded and unhealthy neighborhood. The two largest clinics in Boston, one devoted entirely to tuberculosis and the other, the Massachusetts General Hospital where I do my own work, are at great variance as to what constitutes tuberculosis.

Again, what do we mean by the words "treated as such" that I used above? To one group of physicians, prominent among whom I might name Dr. Joseph H. Pratt, of Boston, adequate and proper treatment means home treatment. To others, and I place myself in this group, home treatment alone is not adequate and proper treatment. We believe that sanatorium treatment, for a while at least, is essential for the average patient's welfare. Dr. Pratt and others apparently are of the opinion that our sanatoria are but a waste of money, while I, having been intimately connected with their management in Massachusetts during the past ten years, naturally have a different opinion. And so I might go on indefinitely.

I do not believe that any one point of view either in diagnosis or treatment will prove to be entirely correct. Each one of us must be willing to give way and if necessary to compromise with what he believes to be the ideal course, or else little progress will be made. I do not like to compromise, nor do I like halfway measures of any kind. On the other hand, half a loaf is better than none, and it is better for a patient to do something that is right, even if it falls far short of the ideal, than to do nothing at all. It is all very well for the physician to demand

complete evidence before making his diagnosis, and having made it, demand that every consumptive do something radical, give up his work, break up his family and go to a sanatorium or elsewhere, but in many cases the physician must be content with less than this or else he will accomplish little.

Some time ago, Boston was deprived of the first and best municipal open-air school in the country, simply, I believe, because a certain gentleman was unwilling to compromise. He apparently reasoned that because this one school was quite insufficient for the needs of a city the size of Boston, it should be done away with. He promised us a complete and ideal system of open-air schools, but this system has never materialized.

Again, for the past five years, a certain city has been urgently in need of a proper tuberculosis hospital. A site was available and suited for the purpose for which the hospital was planned, *i. e.*, advanced consumptives and emergency cases. It was not an ideal site in many respects, but for various reasons seemed adequate. Rather than compromise on this point or depart from his high ideals, this gentleman has been the greatest factor that I know of in preventing any active steps being taken up to the present in building a new hospital. It would be interesting to know how many new cases of consumption have developed during this period because of inadequate hospital provision, and to calculate what the care of these unnecessary cases has cost this city both in money and in unhappiness.

As far as individuals are concerned, the "no compromise" attitude, in my own case at least, would mean that as soon as I had made a definite diagnosis I should advise every patient to give up everything and to go away to a sanatorium at once. This would be quite regardless of necessary sacrifices such as breaking up the family, giving up work, and providing funds. Perhaps if we all assumed this attitude we might cure or arrest a few more consumptives than we are now doing. I doubt very much if the good we would accomplish, however, in this way, would offset the harm we would do in other ways. The following are a few striking cases in point:

1. A. Z., a Lithuanian of 29, married, with one small child, came to see me with a history of a sudden and fairly profuse hemorrhage. Family and previous history were absolutely negative. He was a strong, finely built man looking the picture of health. Physical examination showed what appeared to be a typical tuberculous process at the right apex, with dullness, slight alteration of breath and voice sounds, and râles. Aside from this I could find nothing abnormal. The blood, urine, temperature and pulse were normal, while the sputum showed no tubercle bacilli, but many organisms inside and outside the cells resembling those of influenza. Here was enough evidence to justify a diagnosis of incipient pulmonary tuberculosis and to treat the patient as such. This would have meant making him give up his work and as soon as possible go to a sanatorium. Despite this, I felt this case would bear watching. I saw him again two weeks later and found the process at the right apex apparently clearing up. At a still later visit it had practically disappeared, nor had there been any more blood. As before, the patient felt perfectly well and under my observation has continued so.

In this case a compromise seemed justified, both as to diagnosis and treatment. It would have been very easy to have stamped this man as a consumptive and to have entirely upset his life, for the time being at least. While I do not feel that this is altogether a safe course to advise general practitioners and students to pursue in that a hemorrhage with signs at one apex is usually enough on which to make a positive diagnosis, I am glad that in this particular case I acted as I did.

2. I. L. This patient was a Hebrew of 33, married, who had just started out in business for himself. He had a very early tuberculous process first called to notice by a hemorrhage. His physical condition was excellent, but he was much depressed and worried. I doubt if I could have persuaded him to go to a sanatorium at first, even had I advised it. Instead of doing this, however, I did my best to encourage him and cheer him up.

I allowed him to keep general supervision over his store, but made him shorten his hours and report to me at frequent intervals. He has recently of his own accord decided to quit work for a while and go away for a rest. He is steadily improving and has recently gone back to work.

Here the diagnosis of tuberculosis was never in doubt. So far, however, events have justified me in not making this man give up everything in order to go to a sanatorium. Time alone will show whether or not I have advised him wisely. Were he of a lower grade of intelligence, or were there any constitutional signs or symptoms, I should undoubtedly have acted differently.

3. N. B. D. This patient, an intelligent man of 35, married, with two children and in comfortable circumstances, consulted me in June, 1916. My diagnosis at that time was incipient phthisis, although the sputum was repeatedly negative. There was a slightly elevated pulse and temperature and a few definite and persistent râles at the right apex.

He lived outside the city with every facility for outdoor sleeping and proper hygiene. His work was not hard. I told him that I should place him on trial for a month and if at the end of that time he were not better he must take more radical measures. I have seen him every month up to the present, during which time he has steadily improved, and has gained weight from 148 to over 160 lbs. In January, 1917, he caught cold and his sputum became positive for the first time, while the moisture at the right apex increased. I have talked with absolute frankness to this man and know that he understands the situation perfectly and fully realizes the chances he is taking. Recently and entirely of his own accord he has gone to Saranac to stay as long as necessary.

An older physician, and one of the most eminent in the state, has seen this patient in consultation and fully approves of the course I have pursued. Indeed, he told me that he would consider it a calamity if he were made to break up his home and go away. On the other hand I should consider it a greater

calamity if he were to break down and in addition infect his children while under my care. This is a border-line case, and one concerning which I am frankly in doubt. Were he single and unattached, I should certainly have advised him to go to Saranac or elsewhere at once, but I am sure that he would not have gone; as things have turned out, this is very satisfactorily settled.

4. M. L. This patient, an unmarried woman of 45, had taught young children in a large public school for many years. She had had many burdens to bear outside of her school work and had carried a heavy load of financial worry and responsibility. Despite this, she felt very well and consulted her physician more on account of her friends' wishes than for her own sake. Examination showed what was probably a chronic tuberculous process at both apices, dullness, altered voice and breath sounds and, occasionally, a few dry squeaks. The scanty amount of sputum she was able to raise was negative. She had no cough. Temperature and pulse taken over a considerable period of time at home were normal. The patient herself, except for being rather tired, felt perfectly well. I found this a difficult problem. The local physician who had sent her to me did not like to take the responsibility of allowing her to continue her work. I took the matter up with the local health board. It was finally decided, first, that the case was not to be officially reported, as this would at once make it necessary for her to give up her work, and second, that she was to report regularly every month to me and that the minute any active signs developed, such as cough, sputum, etc., or as soon as I considered her in any way a danger to the children, she must give up teaching. This was made clear to the patient and to all concerned. So far she has done very well. Recently she has obtained leave of absence for a month and is going away, somewhat against her will, for a vacation, which she has not had for five years. My last examination recently made shows the same signs in the lungs, a negative sputum, and excellent general condition. She plans to resume her work in the fall, providing I allow her to do so.

I do not doubt but that there are many physicians who consider the course I have followed in this instance to be radically wrong. Indeed, ten years ago I frankly admit I should have thought so myself. At present, however, I believe it to be a justifiable one, both from the point of view of the patient, who has sufficient intelligence to understand the situation thoroughly and to coöperate in every way, as well as in regard to the children, whom I do not consider to be running any risk.

5. G. H. This patient, a married woman of 42, consulted me in September, 1915. There was a history of a profuse hemorrhage of two or three cups of blood the previous day, following which she took a fairly strict outdoor rest treatment for a few months. Sputum had been repeatedly negative. Over the upper two thirds of the left lung there was some dullness, numerous râles and dry squeaks. Temperature and pulse were normal. My provisional diagnosis was tuberculosis, but I was unwilling to send the patient away or to do anything more radical than to make her lead a sensible quiet life at home. There were no children. I have seen her at regular intervals up to the present time. Always the sputum has been negative, while the process in the lungs is steadily clearing up. The general condition has been excellent during this entire period. I have finally come to the conclusion that this is not a case of tuberculosis but rather one of apical bronchiectasis.

As things have turned out, I feel very glad now that I did not send this patient away to a sanatorium. Physically, such a course would have done no harm and possibly some good. From the social and mental standpoint, however, I am sure that it would have done harm if she had been definitely stamped as a consumptive.

6. Peter P. A wealthy young Italian was referred to me in September, 1916, with a positive diagnosis of pulmonary tuberculosis and a positive sputum. The signs in the lungs were slight, and the general physical condition was very good. He was extremely depressed and worried, however, so much so that

were it not for an older brother of whom he was very fond he would have preferred to do nothing at all for his health, and would have rather welcomed an early death. Three or four years previous, he had been excessively alcoholic; in addition, a neglected luetic infection had affected the central nervous system, causing various distressing symptoms. Although this man certainly knows that he has tuberculosis, and except for going to a sanatorium is living a sensible, proper life in every way, I have never in his presence used the words "tuberculosis" or "consumption" at any time and do not intend to do so. It has seemed to me that he has enough to combat as it is without adding to his burdens. At present he is in California, where he went ostensibly on a business trip. As a matter of fact, it is more for health than business that he has gone, and of this he is perfectly aware. Dr. George Evans, of San Francisco, in a recent letter states that this patient came to him for a moderate hemorrhage. When he returns I shall be interested in finding out what course will be best to pursue under these circumstances.

7. J. D. This patient is a married man of 45 and a clerk for many years in an office where the hours are favorable and where he works under the best hygienic conditions. He was referred to me because he had been refused life insurance on the ground that he was below normal weight, and because of certain suspicious signs in the lungs. He felt practically the same at the time of my examination as he had been feeling for many years. I found a chronic and very inactive process in the lungs, with a positive sputum. I explained the situation in detail and told him that I would put him on trial and that providing he would see me once a month and would abide by my decision he need not give up his work or take any radical steps at present. I examined his wife, whom I found to be strong and healthy. There were no children. The man for whom he worked willingly allowed him to come to the office an hour later and to leave an hour earlier than usual. So far he has done very well and is gaining in weight and strength.

Here again there are doubtless many who would differ radically from me in the course I have advised in this instance. If the disease had shown any signs of activity in the way of temperature and pulse, and indeed, if he had been younger, I should certainly have made him give up his work and devote his entire attention to his health. Again, were he financially differently situated I should likewise have advised his giving up everything. I believe that the course I have advised, however, is the best one in this individual case, but I am quite ready at any time to change my ideas should events prove that I am wrong.

Recently I was asked to prepare some minimum diagnostic standards for cases of tuberculosis with negative sputum for the purpose of the Framingham experiment. I found this a most difficult task. The final draft as accepted by the committee of which I was a member is as follows:

DEFINITIONS.

1. "Loss of weight." By "loss of weight" should be understood an unexplained loss of at least 5 per cent. below normal limits for that particular individual within four months' time.

2. "Loss of strength." By "loss of strength" in its pathological sense is meant an ease of tire which is unusual for that individual patient and which cannot be satisfactorily explained.

3. "Fever." An occasional temperature of 99 should not be considered "fever." A temperature which persistently runs over 99 when taken at least four times a day over a period of one week should be considered of significance and to constitute fever.

4. "Elevation of pulse." Where the average normal pulse of the patient is already known, an elevation of 15 beats per minute when the pulse is taken quietly at home during various periods of the day should be considered abnormal. In cases where the average pulse is not known, and of course this constitutes the majority of cases, we should consider an average pulse of 85 or over in men and 90 or over in women to be abnormal. The combination of a subnormal temperature and an elevated

pulse as defined here should be considered of great importance.

5. "Hemorrhage." Any amount of blood, with or without sputum, requires medical investigation as to its course. Blood streaks, blood spots, etc., may or may not mean tuberculosis. On the other hand a hemorrhage of one or two teaspoonfuls is presumptive evidence of the disease.

6. "Family history." An occasional case of tuberculosis in the patient's uncles, aunts, cousins, etc., should not be considered of importance. It is an important fact when the patient's immediate relatives, such as father, mother, or grandparents, have been tuberculous.

7. "Exposure." Childhood exposure, no matter how slight, is of the greatest importance. Moderate exposure among normal, healthy adults of cleanly habits is of less importance. Of course, prolonged contact with unhygienic habits or surroundings may be a dangerous factor at any age.

8. "Cough." No cough is characteristic of tuberculosis. A persistent cough for six weeks requires investigation. Tuberculosis can exist without any cough whatsoever.

9. "Sputum." The presence of sputum is not necessary for a positive diagnosis. Constant sputum, with or without cough, requires investigation. A positive sputum following one or several laboratory examinations is not to be required for positive diagnosis.

10. "Hoarseness." Any hoarseness or a persistent "huskiness" requires investigation.

On a basis of these definitions the following minimum standards in the diagnosis of active tuberculosis have been formulated:

11. When the constitutional signs and symptoms and definite past history are absent or nearly so, there should be demanded definite signs in the lungs, including persistent râles at one or both apices. By "persistent" it is meant that the râles must be present after cough at two or more examinations, and that the patient must be under observation at least a month.

12. In the presence of constitutional signs and symptoms

such as loss of weight and strength, etc., as defined above, there should be demanded some abnormality in the lungs, but not necessarily râles.

13. A process at the apices should be considered tuberculous, and a process at the base to be non-tuberculous until the contrary is proved.

14. A hemorrhage as defined above is evidence of active pulmonary tuberculosis until the contrary is proved.

15. One should consider a typical pleurisy with effusion as presumptive evidence of tuberculosis. One should not consider a dry pleurisy as conclusive evidence of active tuberculosis without other signs and symptoms.

16. Pain in the chest and shoulders, night sweats, digestive disorders, etc., may be present, and should be investigated.

17. In every doubtful case one should demand that the patient be kept under observation for at least one month, with repeated sputum examinations before making a definite diagnosis.

It remains to be seen whether or not these diagnostic standards will prove satisfactory for the Framingham experiment and for general purposes. At all events, it is undoubtedly true that there must be many cases similar to the ones I have cited above, that are exceptions to these rules and with whom compromises are necessary and justifiable.

DISCUSSION.

Dr. MINOR: Dr. Hawes has brought up a very important subject, because so many men nowadays think that physical signs in themselves are enough to make a diagnosis of active pulmonary tuberculosis. I think that every one of us working in this line feels more and more that in a diagnosis of clinical tuberculosis the physical signs are only one of many things—that the history is fully as important—that the clinical study of the case is fully as important.

By some chance we examine a patient who is in no way suffering from any demonstrable sickness but shows only such incipient signs as Dr. H. has referred to. I think we are in no way justified in telling that patient that he has tuberculosis. It may be true that he has a

latent tuberculosis, but we know how many people go for years with a lesion in their lungs and yet have no real sickness. On the other hand, I think that the publication of one statement Dr. Hawes made is apt to do harm. He said he had not told this young Italian he had tuberculosis. One thing that I believe is essential is that the patient must know he has tuberculosis, though the telling must be tactful, gradual and cautious. There are exceptional instances of course. Hopeless cases need not be told—but many writers have stressed that. And while I fully understand Dr. Hawes's attitude in his case, certainly if his statement goes broadcast it will increase an unfortunate tendency to conceal the facts from the patient. The patient cannot make the necessary sacrifice unless he knows what it is for. And personally my experience has been that it is possible to tell any patient with tuberculosis, except those who are extremely neurotic, that he has tuberculosis—gradually and tactfully to make the nature of tuberculosis known to him so that it will not terrify him. So far as Dr. Hawes's case is concerned I can realize how this distressing news conveyed to that young Italian might badly affect him, because the word tuberculosis connotes to him a different thing from what it connotes to you and me; and I think it is up to the doctor to see that it means to the patient what it means to the doctor. Then it will not frighten him. It means to him certain death. But as long as that is not what it means to you and me, as long as we realize what the situation is and that it is utterly impossible to fight that situation without aid on the patient's part, I think that to withhold the nature of his disease from the patient is a very dangerous teaching. You might do it in extreme cases, but if it is to be the practice I think we are going to start on a dangerous way.

I do feel, although I am a firm believer in the accuracy of the regular methods of diagnosis, that there is a tendency to make them the first and primary thing, which they are not. A diagnosis is the building up of an opinion out of a synthesis (?) of different observations. I know you will agree with me that the information obtained by any intelligent physician from a thoroughly intelligent study of the symptoms is fully as valuable as information obtained from the physical examination. That is the difficulty, I think, with many doctors—they tend to stress too much the physical findings in the case. The man who specializes in this disease more and more lays stress upon symptoms and less stress—not a little, but less—upon physical signs. You are seeking for information, and I think the thing you want to consider is that each one of the factors is important.

Incidentally I might mention one thing that Dr. Hawes omitted and that I consider has a considerable bearing on the diagnosis of tuberculosis—that is the relation of fistula in and to tuberculosis. This is seldom mentioned in the different classifications, and yet I think

it has nearly as much value as a history of pleurisy, and might very well be included in a consideration of the case.

Dr. POTTENGER: Dr. Hawes's paper brought up so many things that ought to be discussed that I will take time only for a few words. First, regarding home or sanatorium treatment. Since we have the conception that we have today about tuberculosis, that there is no absolute specific cure, but that it is a question of raising the patient's resisting power high enough, doing enough things, we have to decide this in individual cases. People can get well at home and in institutions. The greatest difficulty is to realize the fact, or make them realize that the patients themselves are more important than the disease. In handling the case you must create an atmosphere that is helpful. In the home everything is antagonistic; the patient is the exception; while at the institution he is naturally in an atmosphere of helpfulness. Therefore I have the feeling that it is much easier for the patient to get well in an institution.

Referring to Dr. Hawes's attitude in these cases, if we take the statistics of Hart ..... we will have to say that about half the people who reach adult life have an extension from the root of the lung to the pulmonary tissue itself. Of these about one third become arrested· that means that one third of adults in whom the disease exists will practically get well and not know they had it; but two thirds of adults having pulmonary involvement will have actual disease that interferes with their life, one half of which will probably drift into active clinical tuberculosis—which shows that these patients did have considerable opportunity of getting well. I do not doubt that a number of cases treated for neurasthenia are tuberculous and sanatorium treatment will cure many of them. But in many cases we can't give patients sanatorium treatment and then we have to do enough other things to raise their resisting power and bring up the balance.

Dr. WILLIAMS: Dr. Hawes has brought up a very important subject. I think we none of us quite realize the situation, but certain of us are endeavoring to bring it into the limelight sufficiently to insist upon certain things. As Moxan (?) differentiated a good many years ago, there are two things—the man who has got the disease and the disease that has got the man. I have permitted myself in describing the state I shall touch upon to use the term "reconstructive therapeutics"—and this applies to the man who has got the disease—to raise him to a higher plane of efficiency to overcome it. Regarding the matter of tuberculosis from the psychological standpoint here we have the keynote of the situation which Dr. Hawes and Dr. Pottenger have touched upon. I think it would be well if this Association, having the double name of Climatological and Clinical and

dealing with all the problems concerning the illness of the individual, would give perhaps a little more attention than it does to the general problem of raising the index of vigor of the individual—not only in the direction of the tuberculous element but of the disease of localized arteriosclerosis. In our problem there are two or three very important factors—and among others is the question of focal sepsis. We will sooner or later get to a place where our methods of observation and therapeutic economies will be very much better. I will give one instance of the many directions in which we can improve the question of the individual vigor. In the course of keeping papers and clippings, etc., on the whole subject I am working on I have gotten one clipping of a series of observations made in a large industry where 90 per cent. of the employees, presumably healthy men, were found to be suffering from disturbances from the mouth and teeth, which the report went on to say were sufficient to impair seriously the quality of their work. That may or may not be true, but it is a straw which shows the way the wind blows, and that we are very far from doing what we ought to do as clinicians to remove a number of conditions capable of removal, whereby the whole status of the individual can be raised and cases of tuberculosis made much less.

Dr. H. L. Taylor: I believe thoroughly that a patient who goes to a sanatorium has a very much better opportunity of recovery than if he stays at home; but I do not think we ought to allow patients to stay home when there are children in the family. I believe that this is a point upon which there should be no compromise. In St. Paul, where we have really no tenement house population, we have found that all the children of our dispensary patients are infected. And if the tuberculin test is of any value at all it has been in these cases. We take these children to a preventorium and get them in a better condition. I might say that we do not allow them to leave the preventorium in two, three or four months, but keep them there at least eighteen months, for we do not believe that we can bring about such a complete change as the development of an immunity against Tuberculosis in two, three or four months sufficient to render the individual unlikely to have a relapse and return to a condition just as bad as when taken away from the family. There is one complication of this question, that of allowing patients not to go to a sanatorium, on which I think there should be no compromise, and that complication is the presence of small children in the patient's home.

Dr. Dunn: I have only a few words to say in the discussion of this very excellent paper. The thought has occurred to me many times that those of us who are particularly interested in tuberculosis do not approach our patients with an open mind. When we approach

them with the idea of finding out what is the matter with them—not whether or not they have tuberculosis—I believe we shall get along very much better. I think there is too great a tendency to approach a patient with just one idea—has he tuberculosis or has he not?—rather than with the idea of what is the matter with him. There has been a good deal said about telling the patient what is the matter with him. My own position is that, irrespective of what the effect may be upon the individual—perhaps there may be exceptions, the case referred may be exceptional, but it is the exception that proves the rule—the patient must know he has tuberculosis. You cannot expect an intelligent man to take up an intelligent fight without an intelligent idea of why he is doing it, and I have yet to see the patient who was not told what was the matter with him who made a successful fight against tuberculosis.

Dr. Probst: I was much interested in Dr. Hawes's paper. I think it is unnecessary to send every patient to a sanatorium. I expect to meet with objections to my next statement—that we occosionally come across cases of tuberculosis where it is not necessary for the patient to stop work. It is a serious thing sometimes to stop a man from working to support his family, and I personally have seen patients with tuberculosis, where the proof was indubitable, who with a limited amount of work have made good recoveries at home and have supported their families—and I do not think that we ought to have any such rule that every patient must go to a sanatorium. I was much pleased with what Dr. Minor said—that every individual who is tuberculous, unless it is a dying case, certainly must be told that he has tuberculosis.

Dr. Hance: After listening to Dr. Hawes's paper there is one point I should like to speak of, and that is the compromise of the general practitioner with his patients. Patients come here to Lakewood who have not been told they have tuberculosis, and it is up to me to tell them, and I take the general position that every patient should be told that he or she has tuberculosis. When exceptions are made some member of the family should be told the exact condition.

In regard to another question which Dr. Hawes has brought up, I have had a typical case. A young man working in the carpet works at Freehold, where the conditions are such that the State Board of Health had to look into them, had lost 18 or 20 pounds in weight, had had fever, and upon physical examination one would certainly make a diagnosis of tuberculosis. I watched him for three or four weeks while living at home. His temperature became normal, and the condition in his chest cleared up, so that I doubt if even an expert would ld be able to hear any physical sounds in the chest; and I do not feel that that young man had tuberculosis. His sputum was examined

and came back negative. Now if I had told him he had tuberculosis I certainly would have injured his chances of rapid improvement. In nine cases out of ten I try to put the matter clearly to the patient, to get them to do the right thing by going away, but there are individual cases where the circumstances, after the physician has studied the whole thing, the home surroundings, etc., are such that he decides that he can treat the patient at home under the most careful observation—to compromise for five or six weeks under these conditions is justifiable. I do not think we can say positively that every patient must go away to a sanatorium or health resort.

Dr. PERKINS: This is a subject I have had in mind for a number of years. For 17 years I have been in charge of the tuberculosis clinic at the Rhode Island Hospital, which is the second largest general hospital in New England. I learned long ago that we got many patients in the out-patient department referred from the medical clinic, who had signs of tuberculosis, and who presented a clinical history of tuberculosis, that cleared up and showed that they were not tuberculous patients. And as the years have gone on I have seen more and more such cases. In some I have found out what was the cause of the condition. But we do get signs and a clinical history which simulate tuberculosis very closely, and my feeling is that we ought to be very careful in making a diagnosis of tuberculosis in a short time. If the signs persist—and I agree with the men who advocate making the diagnosis on certain signs—then we should say the patient has tuberculosis, but unless there are tubercle bacilli in the sputum I do not think we should do so until the patient has been under observation for some time.

I have been much interested in the use of the X-ray. Here is one condition in which I think the X-ray is of great value. Often where there is some trouble in the chest, perhaps occasioned by hæmorrhage, and we get an X-ray showing an old tuberculous lesion of the lung, I think that under those circumstances we can feel that that case is not one of acute tuberculosis but that there is an old tuberculous lesion and on top of that some weakening of one of the arteries, which has given us the hæmorrhage. A case like that in advice and treatment is different from what it would be if the hæmorrhage were from an acute condition. I believe that these patients will get along perfectly well without being sent to a sanatorium. I also oppose, until we can have a fairly positive diagnosis, telling people definitely that they have tuberculosis, and sending them away from home. I sent you all a reprint on this subject, in which I expressed the feeling that had been pent up for quite a number of years. It was not written on the spur of the moment, but because I thoroughly believed what I wrote. I think an immense amount of harm has been done by sending patients away with the diagnosis

of tuberculosis, making them give up their business, sending people away from school and college and making them give up their life work on an insufficient diagnosis, which in many cases afterwards proves not to have been correct. One thing to consider in making a differential diagnosis is chronic appendicitis. We can have with chronic appendicitis symptoms which so far as they go more closely simulate tuberculosis than any other condition I have found. I know of one patient who went to Saranac Lake for tuberculosis, but who could not get rid of the fever, a little low-grade fever. He came back and a short time afterwards the cause of the trouble was revealed by an operation for appendicitis, which was followed by disappearance of the symptoms.

Dr. ELLIOTT: Most of Dr. Hawes's paper does not lend itself to discussion—we can accept it. But I think he takes the wrong point of view in discussing what we shall do with our patients when the diagnosis is once made. I do not think we should bow to Dr. Hawes's dictum that the patient must go to a sanatorium. The experience I have had in sanatorium and city practice leads me to look at the matter from the opposite point of view. When the diagnosis has been made let us consider the question, can we treat this patient at home? and if we cannot satisfy ourselves that we can treat him satisfactorily at home then let us send him to a sanatorium. I am perfectly satisfied that a large majority of the cases diagnosed as tuberculosis can be treated equally satisfactorily at home. That, of course, means if the patient has a home. We can't treat a man in a room in a boarding house, and this is true not only of patients in private practice but of those in the tuberculosis dispensary. At the General Hospital in Toronto we get a larger proportion of patients coming to our dispensary cured by treatment at home—that is, who lose the physical signs of active disease and return to working capacity—than is shown as the result of sanatorium treatment. And I should like to ask that that view be taken—first, can we treat the patient at home, and if not then send him to a sanatorium.

Dr. BROWNING: I should like to ask Dr. Hawes one question. He spoke of a rise of temperature, and I should like to know if he takes into consideration the subnormal temperature. There are occasions when patients will not show a rising temperature but a moderately low diurnal temperature.

Just one question, too, that was brought out by the last speaker. Many here have spoken as though they were able to control their patients—they have spoken of the ideal. If the patients are wealthy and able to do as they like, or if they are poor and live in a very wealthy community that has provided conditions which permit the patients to be dealt with in an ideal way you may be able to get them

to do as you wish. In a community, where there are probably 5,000 tuberculous individuals who have demonstrable tuberculosis, which represents considerably more in contacts, what are you going to do with them? How can you put them in an institution? Certain ones you can be put in an institution and certain ones you cannot. And with the ones you cannot the treatment resolves itself into following up the patient in the home, by your school nurse and your dispensary nurse, and making the home conditions such that the contacts become as nearly as possible immune. There is this large number of people who have to be considered.

Dr. HAWES (closing): I don't want Dr. Minor or anybody else to think I don't tell my patients that they have tuberculosis. In the particular case I cited the man was so down and out and had such marked suicidal tendencies that I didn't see the point in frightening him with the diagnosis.

I think that where there are children the sanatorium ought to be insisted upon. But suppose the patient tells you to go to thunder, as in one of my cases. The man said "I will not go to a sanatorium," but in this particular case by keeping at his wife I finally got him to go away.

I guess Dr. Elliott and I are going to disagree on the subject of home treatment.

In regard to subnormal temperature, I simply spoke of temperature along with rapid pulse. I am indebted to you gentlemen for helping out in what has been a difficult problem.

THE AMERICAN VIRGIN ISLANDS, OUR NEWEST WEST INDIA POSSESSIONS; THEIR CLIMATE AND TOPOGRAPHY.

By GUY HINSDALE, A.M., M.D.,

Secretary of the American Climatological Association; Associate Professor of Climatology in the University of Pennsylvania, etc.

These islands have now been ceded to the United States after negotiations with the Danish government carried on at intervals during the past fifty years. In 1867 we agreed to buy them for five million dollars, but the sale finally fell through at Copenhagen. They will now cost us twenty-five million dollars as agreed upon by treaty January 17, 1917, when title passed. Formal actual possession took place March 31 of the present year, when the purchase price was paid. The control of these islands will be under the Navy Department, although our other island possessions are under the War Department. This on account of their importance as a naval base.

These islands about fifty in number, all told, are east of Porto Rico and have a combined area of 138 square miles; St. Thomas, the best known, covers 28 square miles; St. John, 20 and St. Croix, 84. They lie between 18 and 19 degrees of north latitude at 65 degrees west longitude.

It is remarkable how little is known in the United States about these islands which were named Virgin Islands in the days o Columbus. It was then customary to name newly discovered territory after the saint's day in the calendar when the discovery was made. I wrote to the Bureau of Insular Affairs at Washington for information. In the reply the only reference to the climate is that it is very even with extremes of 60 and 90 degrees Fahrenheit (15.6–32 C.). It seems that there are about 33,000 people there, nearly all negroes.

When I wrote to Washington I did not know that another had preceded me by exactly fifty years—no less a personage than the late Mark Twain. His letter is doubtless on file in the State Department and is dated December 10, 1867. It has unusual interest at the present juncture, especially as these little jewels of the Antilles have fetched so high a price.

Mark Twain wrote: "Could you give me any information respecting such islands, if any, as the Government is going to purchase? It is an uncle of mine who wants to know. He is an industrious man and wants to make a living in an honest, humble way but more especially he wants to be quiet. He wishes to settle down and be quiet and unostentatious. He has been to the new island St. Thomas, but he says he thinks things are unsettled there. He went there early with an attaché of the State Department who was sent down with money to pay for the island. My uncle had his money in the same box and so when they went ashore, getting a receipt, the sailors broke open the box and took all the money, not making any distinction between Government money, which was legitimate money to be stolen, and my uncle's money, which was his own private property and should have been respected. But he came home and got some more and went back and then he took the fever. There are seven kinds of fever down there you know and as his blood was out of order by loss of sleep and general wear and tear of mind he failed to cure the first fever and somehow he got the other six. He is not the man that enjoys fevers, though he is well meaning and always does what he thinks is right, so was a good deal annoyed when it appeared he was going to die; but he worried through and got well and started a farm. He fenced it in and the next day that great storm came and washed the most of it over to Gibraltar or around there somewhere. He only said in his patient way that it was gone and he wouldn't bother about trying to find out where it went to, though it was his opinion it went to Gibraltar.

"Then he invested in a mountain and started a farm up

FIG. 1—View of Charlotte Amalia, St. Thomas, looking west from the Frederiksberg. (Photo by Clarence Taylor.)

(FIGS. 1-6 from the *Geographical Review*, November, 1917. Courtesy of the American Geographical Society, New York.)

FIG. 2—Panoramic view of St. Thomas harbor and town of Charlotte Amalia, looking southward. (Photo by Theodoor de Booy.)

FIG. 3.

FIG. 4.

FIG. 3—Coral Bay, St. John. Tortola Island (British) in background. The white buildings are the Moravian Missionary settlement of Emmaus. (Photo by Theodoor de Booy.)

While Coral Bay, with its complete shelter from storms, its depth of water, and its numerous inlets, offers better facilities for a naval base than does St. Thomas harbor, its position with respect to the mountains of Tortola Island, upon which guns of heavy caliber could be mounted, would jeopardize the safety of a fleet.

FIG. 4—St. Thomas harbor looking eastward from foot of Careen Hill. (Photo by Clarence Taylor.)

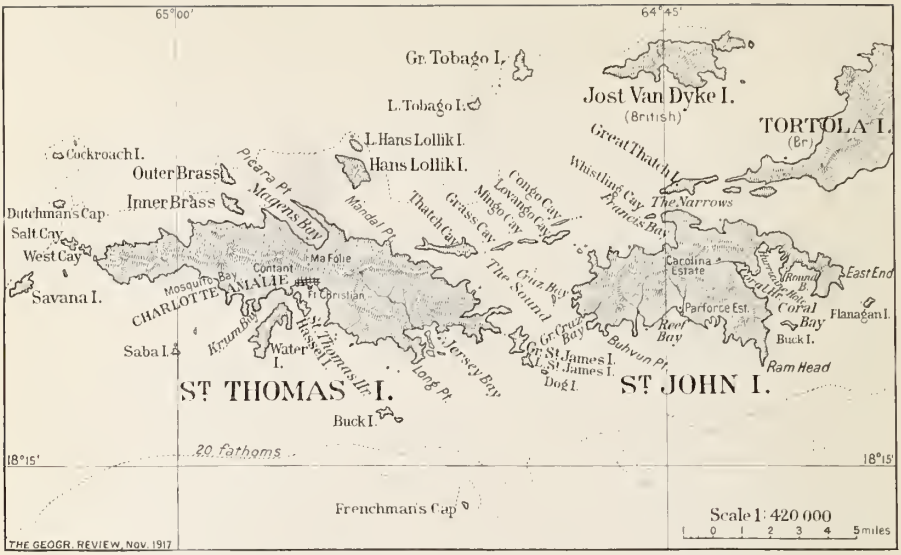

FIG. 5—Sketch map of the islands of St. Thomas and St. John. 1:420,000.

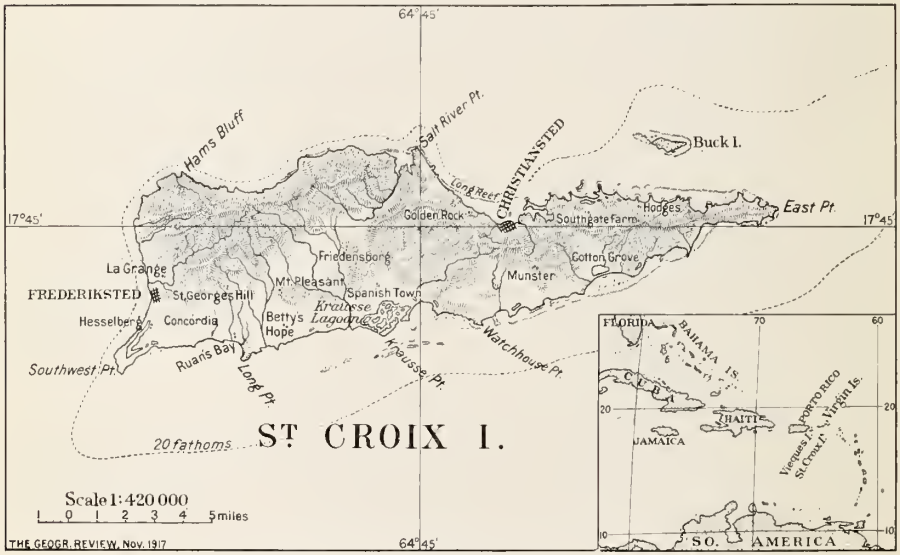

FIG. 6—Sketch map of the island of St. Croix. 1:420,000

there so as to be out of the way when the sea came ashore again. It was a good mountain and a good farm but it wasn't any use; an earthquake came the next night and shook it all down. It was all fragments, you know, and so mixed up with another man's property he could not tell which were his fragments without going to law; he would not do that, because his main object in going to St. Thomas was to be quiet. All that he wanted was to settle down and be quiet.

"He thought it all over and finally he thought he would try the low ground again, especially as he wanted to start a brickyard this time. He bought a flat and put out a hundred thousand bricks to dry preparatory to baking them. But luck appeared to be against him. A volcano shoved itself through there that night and elevated his brickyard about two thousand feet in the air. It irritated him a good deal. He has been up there and he says the bricks are all baked right enough but he can't get them down. At first he thought maybe the Government would get the bricks down for him, because, since the Government bought the island, it ought to protect the property where a man has invested in good faith; but all he wants is quiet and so he is not going to apply for the subsidy he was thinking about.

"He went back there last week in a couple of ships of war to prospect around the coast for a safe place for a farm where he could be quiet, but a great tidal wave came and hoisted both of the ships out into one of the interior counties and he came near losing his life; so he has given up prospecting in a ship and is discouraged. He is getting to think that St. Thomas is not quiet enough for a man of his turn of mind and that is why he wishes me to find out if Government is like to buy some more islands shortly. How is Porto Rico for his style of man? Do you think the Government will buy it?"

Access. The Quebec Steamship Company's steamers sail twice a month from New York. Distance 1440 miles. The distance to Panama is 1000 miles.

To San Juan, Porto Rico, the time is $7\frac{1}{2}$ hours by boat.

From St. Thomas to St. Croix is 42 miles and there is a weekly steamer making the round trip. Fare $3.00 one way. The oversea traffic of St. Thomas is considerable. In 1913, 200 vessels stopped there and eight steamship lines make regular calls.

Products: Sugar, rum and molasses are important. St. Thomas and St. John produce the best of bay oil, from which the bay rum is made; also lime juice and other lime-tree products. The bay tree (*Pimenta acris*) grows profusely in certain parts of the island of St. John. The leaves are picked by children, who climb the trees and throw them down to women, who pack them in bags. The leaves are picked from each tree about once a year. These leaves are then brought to the distillery where they bring 2 cents a pound. The distilled oil is worth about $5.00 a quart. By adding alcohol 150 quarts of bay rum can be made from one quart of the oil. The process of distillation and manufacture is carried on at St. Thomas while the bay leaves are grown in St. John.

St. Thomas was devastated August 7, 1899, by a terrific hurricane, when 3,000 lives were lost, principally by drowning, the barometer falling to 27.5 inches. Another terrific hurricane occurred October 9, 1916.

The most serious volcanic disturbance was the eruption of Mt. Pelee, when the City of St. Pierre and vicinity were totally destroyed and thousands lost their lives.

MEAN TEMPERATURES OF STATIONS IN THE LESSER ANTILLES, WEST INDIES.*

Christianstad, St. Croix.

Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept	Oct.	Nov.	Dec.	Annual.
Lat. 17° 45′. Long. 64° 41′. Elevation, not given. Length of record, 28 years.												
77	76	77	78	80	81	82	82	82	81	79	78	79

Basse Terre, St. Christopher.

Lat. 17° 18′. Long. 62° 48′. Elevation, 30 feet. Length of record, not given.												
76	76	76	78	79	80	81	81	81	80	80	77	79

* From U. S. Department of Agriculture, Weather Bureau.

St. Johns, Antigua.

Lat. 17° 5′. Long. 61° 50′. Elevation, 82 feet. Length of record, 12 years.

76 76 76 78 79 80 80 81 80 80 78 77 78

Pointe a Pitre, Guadeloupe.

Lat. 16° 14′. Long. 61° 32′. Elevation, 66 feet. Length of record, 7 years.

74 74 75 78 80 81 81 81 80 79 77 75 78

Camp Jacob, Guadeloupe.

Lat. 16° 1′. Long. 61° 42′. Elevation, 1,749 feet. Length of record, 10 years.

70 68 68 70 72 73 73 74 73 73 71 70 71

Roseau, Dominica.

Lat. 15° 17′. Long. 61° 23′. Elevation, not given. Length of record, 3 years.

77 77 77 79 80 80 80 81 81 80 79 78 79

Fort de France, Martinique.

Lat. 14° 36′. Long. 61° 6′. Elevation, 13 feet. Length of record, 8 years.

76 76 76 78 80 80 80 81 80 79 78 77 78

Bridgetown, Barbadoes.

Lat. 13° 4′. Long. 59° 37′. Elevation, 56 feet. Length of record, 4 years.

78 77 77 79 80 80 80 81 81 80 80 78 79

AVERAGE PRECIPITATION (INCHES) FOR STATIONS IN THE LESSER ANTILLES, WEST INDIES.*

* From U. S. Department of Agriculture, Weather Bureau.

	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Annual
Christianstad, St. Croix. 28 years record.	2.32	1.93	1.22	2.91	4.49	4.61	3.94	4.49	5.75	6.02	5.71	4.17	47.56
Basse Terre, St. Christopher. 44 years record.	3.66	1.89	2.09	3.31	4.17	4.02	4.45	5.67	6.46	6.54	5.35	3.78	51.39
Estate Aromet, St. Christopher. 20 years record.	8.74	4.57	4.13	3.82	6.02	12.17	14.41	12.24	10.91	9.92	10.79	7.36	105.08

St. Johns, Antigua.

34 years record.

3.39	2.28	2.20	3.39	4.68	4.41	5.12	5.24	6.26	5.98	5.39	3.82	52.16

Pointe a Pitre, Guadeloupe.

23 years record.

3.90	3.19	2.99	3.50	6.54	6.30	5.91	6.61	6.14	7.64	7.01	4.65	64.38

Basse Terre, Guadeloupe.

Lat. 16° 0′. Long. 61.4°. 34 years record.

5.98	3.82	2.95	3.86	5.47	6.57	7.48	8.50	7.64	7.99	6.50	6.42	73.18

Camp Jacob, Guadeloupe.

27 years record.

10.16	7.72	7.80	7.40	12.17	14.37	17.36	15.91	15.00	15.55	14.06	10.75	148.25

Roseau, Dominica.

21 years record.

5.83	2.80	2.28	2.40	2.87	8.15	10.47	10.79	8.78	6.93	7.87	5.71	74.88

Fort de France, Martinique.

31 years record.

4.68	4.25	2.91	3.90	4.68	7.44	9.37	10.31	9.25	9.84	7.87	5.91	80.41

St. Pierre, Martinique.

50 years record.

5.67	3.98	4.09	3.50	6.10	9.53	11.97	13.66	10.71	9.09	8.82	7.32	94.44

Santa Lucia.

Lat. 14° 1′. Long. 61° 0′. 19 years record.

5.20	3.27	3.98	3.82	6.97	9.37	10.28	10.39	10.24	10.08	9.84	8.35	91.79

Kingstown, St. Vincent.

Lat. 13° 13′. Long. 61° 13′. 23 years record.

5.08	2.68	2.95	2.91	5.98	9.09	9.13	8.07	8.50	8.86	9.68	5.75	78.68

Barbadoes.

Lat. 13.2°. Long. 59.6°. 25 years record.

3.27	2.60	1.46	2.01	3.54	5.43	5.71	7.24	6.22	8.70	7.09	4.49	57.76

DIFFERENTIAL DIAGNOSIS BETWEEN FUNCTIONAL AND ORGANIC CARDIAC MURMURS.

By DR. COLLINS H. JOHNSTON,

GRAND RAPIDS, MICH.

About a month ago a business man in a neighboring city wished to protect his associates by taking out a fifty thousand dollar life insurance policy. He was rejected because the examining physician reported that he had a heart murmur. He applied to another company and was again rejected for the same reason. He then went east to the home office of another company and was rejected for a third time. He then came to Grand Rapids where I examined him for a fourth company and found him to have a functional instead of an organic heart murmur. I therefore recommended his acceptance. Wishing to have the applicant passed upon by two examiners on account of the size of the policy, the company sent him to Chicago where my finding was confirmed. The policy was accordingly issued.

This gentleman's experience is an illustration of what is happening many times a day, although the outcome in this particular case was rather an unusual one, as most life insurance companies regularly reject all applicants with heart murmurs.

Three years ago I sent a questionnaire to the medical directors of a number of old-line companies and received replies from twenty. Half of these said they accepted applicants with heart murmurs, "if functional," and then proceeded to state that owing to the difficulty of making a differential diagnosis between functional and organic murmurs such cases were regularly rejected.

One director said: "I assume that a functional murmur is nearly as rare as a Jew in an Irish graveyard. I do not believe

it is possible to differentiate between a functional and an organic heart murmur. Functional murmurs do not exist in life insurance."

Another stated that "the only sure differentiation is made by the subsequent history of the individual. If the murmur disappears permanently then the presumption is that it was a functional murmur. As long as the applicant shows a murmur of any kind we do not consider it safe to accept him."

Another director wrote: "I believe that from a clinical standpoint it is possible by physical examination to differentiate between a functional and an organic murmur, but for practical life insurance work it is not always wise to do this."

Many a man has left his family penniless, many a promising business career has been sidetracked, many brilliant boys and girls on graduating from high school have been unable to take advantage of a scholarship that would enable them to go through college, many a young woman has been advised never to marry for fear of being unable to stand the strain of child birth, many men and women have become confirmed neurasthenics and live in constant fear of sudden death, many young men anxious to serve their country in one or another branch of the army or navy at a time when they are so much needed have been prevented from doing so and many more will be—all because so many members of the medical profession still cling to the errors and traditions of the past and do not yet appreciate the fact that the great majority of heart murmurs are physiological and normal signs and do not indicate past or present disease of the heart of any kind.

A few years ago in writing to one of my friends in Philadelphia, a most distinguished clinician, I stated that I thought a little exercise or the assumption of one position or another would produce a murmur in the hearts of over half of the people who came to my office and that I thought most of such hearts were perfectly normal. He replied that he considered any heart pathologic that presented a murmur under any circumstance. Yet it

is necessary to exercise great care in estimating the condition of some of these hearts as illustrated by a case under my observation the past two years. A man fifty-nine years of age came to me complaining of occasional attacks of slight pain in his stomach which seemed to come on after exercise and to have no relation to his meals. He also admitted being a little short of breath on walking up a hill. He looked well and was very active for a man of his age. His palpable arteries were not hard. He had a soft systolic murmur at the apex. The heart seemed a little enlarged and the second pulmonic sound slightly accentuated. The systolic and diastolic blood pressures were within normal limits, but during the taking of them there was noticed some inequality in the strength of the heart beats or unequal force of the systoles of the left ventricle, although there was no arhythmia and no inequality of the pulse at the wrist. The diagnosis of myocardial degeneration and the serious prognosis were laughed at by the patient and ridiculed by three or four doctors who saw the patient later. He made no change in his activities and dropped dead when about to enter his automobile three days before I left home.

Of course there may be times when a prompt and certain diagnosis between functional and organic murmurs cannot be made, but the writer believes that a careful and comprehensive examination will lead to a correct estimate of the condition in the great majority of cases.

All authorities agree that functional murmurs are usually systolic. Cabot states that the majority of murmurs heard over the heart are unassociated with valvular disease and that 99 per cent. of functional murmurs are systolic, the exception being the very rare diastolic functional murmur which may occur in severe anæmia when the hæmoglobin is 25 per cent. or less, probably due to stretching of the aortic ring or to sounds produced in the veins of the neck and transmitted to the vena cava.

The first thing to do when confronted with a murmur is to decide whether it is systolic or diastolic. If diastolic, which also

includes presystolic, the lesion may at once be concluded to be an organic one.

Functional murmurs include anæmic, cardiomuscular, cardiorespiratory, accidental and physiological murmurs. They may be heard in any situation over the heart, but are most frequent in the pulmonary and mitral areas. The vast majority are best heard over the pulmonary area in the second left interspace.

Osler mentions five classes of systolic murmurs in the pulmonary area in which no lesion of the pulmonary valves exists. They are not associated with any evidence of enlargement of the heart nor with accentuation of the pulmonic second sound. The same may be said of all functional murmurs, whatever their origin.

Whenever a systolic murmur is heard over the apex, the tendency is to conclude that a mitral insufficiency is present, but Osler states that "a murmur systolic in character, and of maximum intensity at the apex, and propagated even to the axilla, does not necessarily indicate incompetency of the mitral valve. There is heard in this region a large group of what are termed accidental murmurs, the precise nature of which is still doubtful. They are probably formed in the ventricle and are not associated with hypertrophy or accentuation of the pulmonary second sound."

Functional murmurs may be soft or harsh, loud or weak, long or short, and they may be present over any part of the cardiac area. They are usually not loud and are poorly conducted, but some of the loudest and most widely conducted murmurs I have heard were functional. They may vary with the position of the patient and the phase of respiration. In some cases they are present in the upright position and disappear when lying down, in others the murmur appears only when lying down and disappears when standing up or on slight exertion. Cardiorespiratory murmurs are often loudest at the apex, but may be heard as high as the second rib in front, in the axilla, and below the angle of the scapula but as a rule the area over which they are heard is very limited.

They are usually louder at the end of deep inspiration and frequently disappear altogether on deep expiration. In still others they are persistent under all circumstances.

Organic systolic murmurs occur in mitral and tri-cuspid regurgitation, aortic and pulmonary stenosis. In mitral regurgitation the heart is regularly enlarged, particularly in the transverse diameter, due to hypertrophy of the right and left ventricles. The apex impulse is diffuse and heaving and is displaced to the left and downward, the second pulmonic sound is accentuated. Over the apex is to be heard a systolic murmur which is conducted to the left and heard in the axilla and below the angle of the scapula. Without these three characteristics, murmur at the apex, enlargement of the heart and accentuation of the pulmonary second sound, a diagnosis of mitral regurgitation is not justified.

In tri-cuspid regurgitation, particulary if large, the right auricle is always dilated. This may be even unaccompanied by a murmur.

Aortic stenosis without regurgitation is extremely rare. Several clinicians of wide experience have told me they have never seen a case. When present it gives rise to a marked hypertrophy of the left ventricle. An aortic systolic murmur is not uncommon, however, in later life, as a result of some stiffening or roughening of the cups, unaccompanied by narrowing of the orifice, and the left ventricle is more or less hypertrophied.

Pulmonary stenosis leads to enlargement of the right ventricle, but organic disease of the pulmonary valve is very rare, except in congenital heart disease when the disturbance of the circulation is usually considerable and clubbing of the finger tips, cyanosis and other congenital defects often coëxist. The right heart too is generally enlarged.

One of the most important and constant findings in cases of organic murmur is enlargement of the heart. This seems to be more regularly the case with lesions producing systolic murmurs than with those giving rise to diastolic murmurs. But it cannot

with accuracy be said that valvular lesions *always* lead to hypertrophy. "It is possible to have a valvular lesion at the aortic or mitral orifice without compensatory hypertrophy, but this is rare" (Councilman).

Warthin states that "it is however very unusual for a valvular lesion of long standing and of any great degree of severity at either the aortic or mitral orifice to exist without more or less compensatory hypertrophy. Were it not for the compensatory hypertrophy of the heart walls which usually occurs in these cases, they would seldom terminate in recovery."

Practically all authorities agree that pathologic lesions causing organic systolic murmurs regularly lead to enlargement of the chambers or the walls of the heart or both. The exceptions to this rule are so rare as to be almost negligible in practice. Inasmuch therefore as the differential diagnosis between functional and organic murmurs seems to depend so largely upon the recognition of cardiac hypertrophy, let us consider for a few moments what the clinical signs and symptoms of enlargement of the heart are.

Displacement of the apex beat downward and to the left is an important sign. In healthy adults the apex impulse is usually felt in the fifth interspace immediately inside the nipple, seven to ten cm. from the median line. It may be in the fourth interspace and outside the nipple line in children and some adults (McKenzie).

The extent of the impulse varies, ordinarily it covers an area of about two cm. square. Broadening of the impulse is suggestive of dilatation, or hypertrophy and dilatation combined, but a diagnosis of hypertrophy cannot be made from an increase in the area of pulsation alone, as this may be due to extrinsic causes; nor should a diagnosis be made from displacement of the apex beat alone, as this may be due to other causes, such as adherent pleura, fibroid tuberculosis, pleural effusions or pneumothorax.

The only pathognomonic sign of hypertrophy of the left

ventricle as far as the apex is concerned is *increased force* of the apex beat. A large diffuse beat with a forcible, thrusting, heaving impulse and extension of the area of cardiac dullness to the left are characteristic of hypertrophy and dilatation of the left ventricle. The area of visible pulsation is also increased. In nervous people, the heart often beats rapidly and with an appearance of increased force, but on putting the hand over the heart it will be found that the beat is light and weak.

In case the apex beat cannot be located with the patient on the back, it is well to look for it with the patient on the left side. In some cases the presence of an apex beat in the sixth interspace can be recognized only with the patient in this position and it may be valuable evidence of enlargement of the heart.

It is usually more difficult to determine enlargement of the right heart than of the left by percussion, because the right border is covered more completely by the lung and sternum. Kovacs teaches that when dullness extends one finger's breadth to the right of the sternum, it means dilation of the right ventricle. A heart may be enlarged by hypertrophy of its walls as well as by dilatation of its cavities. We cannot percuss accurately enough to appreciate one to two cm. increase in the size of the heart, which is the usual limit of enlargement in hypertrophy alone. Nor do we in postmortems find a greater degree of hypertrophy than this unless it is accompanied by dilatation (Cabot). Any enlargement of the heart, therefore, which can be demonstrated by percussion must be due in part to dilatation of its cavities whether there be coëxisting increase in the thickness of its walls or not. In the great majority of cases of hypertrophy, from whatever cause, both sides of the heart are affected.

The most common symptoms of organic cardiac disease are undue breathlessness, or fatigue, or a sense of tightness across the chest on or after exertion; palpitation and precordial distress or pain. The distress may be referred to the epigastrium and such patients attribute their trouble to the stomach rather than to the heart. Some patients, however, who deny shortness of

breath on further questioning will admit their inability to walk rapidly or stand unusual exertion with as much freedom from distressing symptoms as formerly. A tendency towards breathlessness on ascending a flight of stairs or walking up a hill are most important symptoms, but are also common to many affections besides heart disease. Slight pain in the precordium is also a frequent symptom of myocardial weakening but often occurs in functional heart affections and is frequently absent in organic disease. The most characteristic feature of pain due to heart disease is that it is brought on by exertion though it may not appear until some hours later. A little swelling around the shoe tops at night or some swelling of the feet is a not uncommon symptom.

Green has also called attention to the astonishing variations in discomfort, pain and hyperesthesia which may result from cardiac disease. Conditions quite remote from the heart itself and attributable to secondary congestions of distant organs, such as pain in the lower axilla and shoulders, in the chest wall, in the epigastrium, in the inner aspect of the upper arm and the ulnar surface of the forearm, wrists and fingers, may be due to weakening of the myocardium. Many of the above symptoms may pass unnoticed until recalled by questions.

In order to determine whether enlargement of the heart could be eliminated by careful physical examination in cases of heart murmur which I considered to be functional in origin, Dr. Henry Hulst has examined twenty-five men for me in the past two years with the X-ray. Each one had a systolic murmur over the heart and had been rejected by life insurance companies on account of it from one to four times. They were selected from a considerable number of cases of heart murmur coming under observation during this period. Usually it is a simple matter to make the differentiation between functional and organic murmurs. As most cases of advanced tuberculosis are readily recognized, so are most cases of organic heart murmur; but it is sometimes as difficult to exclude organic disease in a

heart as incipient tuberculosis in a lung and only debatable cases were sent to Dr. Hulst.

The following will serve as illustrations:

Mr. E. J. H. Thirty-nine years old. Never seriously ill. Height 5 feet, $11\frac{3}{4}$ inches. Weight 150 pounds. Blood pressure 131–80. Force of cardiac impulse normal. Dullness at least one inch within the nipple line. P.m.i. under the fifth rib, 7 cm. from the median line. At the aortic area a soft systolic murmur, taking the place of the first sound. Second sound normal. Pulmonary area same as at aortic. Second pulmonic sound not accentuated. Over the apex a loud systolic murmur with the first sound; conducted downward to the edge of the ribs, and audible all over the front of the chest, right and left, in the right axillary region as well as in the left, and below the right scapula as well as the left. The murmur was more distinct at the end of forced expiration, less distinct but still quite audible at the end of deep inspiration. It was heard equally well in the standing, sitting and lying position. It was the loudest murmur I ever listened to and could be heard with the ear removed an inch or more from the chest wall. X-ray examination showed no hypertrophy of the heart.

Mr. J. M. Aged 25. Never been ill. Has a loud systolic murmur over the pulmonary area, which is conducted downward to the mitral area and to the right over the aortic area. No accentuation of the second sounds. The peculiar thing about this case was that while the systolic blood pressure was 120, the diastolic pressure was 0. That is, the fifth auscultatory phase persisted down to zero. A good many clinicians consider this to be pathognomonic of aortic regurgitation, although Norris, Faught, Billings and others have seen it occasionally in exophthalmic goiter and other conditions in which vasomotor tone is greatly reduced. There was no diastolic murmur over the heart and the man had never had any symptoms of cardiac insufficiency. The X-ray examination showed the outline and size of the heart to be normal.

Dr. Hulst examined all of these 25 cases with both the fluoroscope and rœntgenograph and in no case was enlargement of the heart discovered. They were all accepted as first class risks for life insurance. For the rœntgenographic examination

the method of Kreuzfuchs was used, which is described in the *Munchener medizinische Wochenschrift* of May 7, 1912, combined with the teleorœntgen method described by Williamson.

As a general rule broad hearts are found in broad chests and narrow hearts in narrow chests. Short, heavy people have relatively broad chests, large hearts and high diaphragms and tall slim people relatively narrow chests and narrow hearts. Kreuzfuchs therefore bases his formula primarily upon the relative length of the greatest transverse diameter of the heart shadow to the transverse diameter of the thorax. But as the usual manner of taking chest exposures gives a considerable degree of distortion and as this false magnification rapidly becomes less with the increase of distance of the tube from the plate, the pictures are taken at a distance of two meters. The rays then become practically parallel and the magnification negligible, amounting to not more than one cm. in each diameter. In order to show the outline of the heart in diastole an exposure of two seconds is made so as to include at least one complete cardiac cycle; and in order that the diaphragm may always be in the same position, the patient is instructed to take a deep breath and hold it.

When the picture is taken the outline of the heart on the right and left sides is marked where it crosses a horizontal line drawn across the thorax through the apex of the cardio-phrenic angle on the right side. It will be seen that this line is thus divided into three parts, the width of the heart occupying the central one. Kreuzfuchs states that the proportionate length of the three parts from right to left should be 8–24, 10–24 and 6–24. The part occupied by the heart shadow is 10–24 of the width of the chest.

Numerous examinations of healthy hearts have shown that even when the size and weight and age of the individual are taken into consideration there are still great variations in the average figures. It is stated by some authorities that while it should be possible to detect enlargements of 2 cm. in the trans-

verse diameter, in many cases enlargement of 3 cm. may occur without exceeding the maximum figures. Groedel says the normal heart size is variable and enlargement should be judged not by millimeters but by centimeters.

To allow therefore for normal physiological variations, a heart is considered normal when the shadow does not occupy more than 11 or 12 twenty-fourths of the entire length of the line. While this formula may not be classed as an absolutely accurate method of determining cardiac hypertrophy, it is convenient, readily used and probably more dependable in its results than orthodiagraphy and the tables of Dietlen and Groedel.

Case has used the Kreuzfuchs formula in combination with the orthodiagraph for determining the size of the heart in 30,000 cases in the last five years. He believes that while the figures obtained by this method are not absolutely exact, it is to all intents and purposes as accurate as the methods of Dietlen and Groedel, the error amounting to less than 1–60, so that for all practical purposes, the Kreuzfuchs formula is sufficient and has the further advantage of being simply and quickly used without the aid of any special apparatus. On the whole he has found that the electrocardiagraphic findings and the Kreuzfuchs formula agree.

The electrocardiagraph, however, is probably the most valuable and accurate method of determining cardiac hypertrophy, but it is unfortunately not accessible to all clinicians.

Mortenson states that during the last three years he has taken about 1,500 tracings and feels justified in saying that the electrocardiagraph practically always shows a left or right hypertrophy when present. He finds that in practically every case which shows a left hypertrophy it can be confirmed by physical examination. He has found that in cases of extensive mitral lesions with physical findings of apparent left hypertrophy, cardiac dullness extending way to the left, the electrocardiagraph may show evidence of a right hypertrophy. In such cases a more careful study has made it probable that a mitral stenosis

was present as the predominating factor in the mitral lesion, causing a preponderance of right ventricular muscle.

A few years ago Lewis of London is said to have made careful dissections of the heart, separating and weighing the muscles of the left and right ventricles, and found that the electrocardiagraphic evidences of hypertrophy in one side or the other practically always tallied with the autopsy findings.

Conclusions.

1. Functional murmurs are practically always systolic.
2. Diastolic murmurs are probably always pathologic.
3. A systolic murmur alone is of no significance and does not indicate valvular disease.
4. The presence or absence of hypertrophy is by far the most important diagnostic factor in differentiating between functional and organic heart murmurs.
5. Absence of symptoms of impairment and a normal cardiac outline on physical and X-ray examination suggest a normal heart.
6. The blood pressure should always be taken.
7. In cases of doubt, the electrocardiagraph may give useful information.

DISCUSSION.

Dr. Dunn: One feature that I think ought to be spoken of is the frequency with which in mountainous countries we find a heart that might be thought to be pathologic and which is not. I think everyone here is familiar with the fact that in mountainous regions, in those individuals who live in the valleys and walk up 2,000 or 3,000 feet a day to do their day's work, coming down again at night, it is customary to have a cardiac area something like a finger's breadth farther out than normal, and it is very hard to say whether an actual hypertrophy exists or not.

Some of us were discussing today the significance of these murmurs, which is going to be a matter of importance in examining troops. I don't doubt that many members of this Association will have an important part to play in that work. A short time ago I

happened to see three men preparatory to their going into the Officers' Training Camp. One had a heart murmur and a history of rheumatism, and I advised him not to go down at all The other two, referred to me by the officer of the Medical Corps, each had a perfectly definite murmur, which I was convinced was purely functional, but I advised them not to go down, on the ground that if they did go they were practically certain, in the present state of medical knowledge in this country regarding heart murmurs, to be rejected. I did not think it right to let them go so far and take the chance of being rejected by someone not perfectly familiar with heart murmurs.

Dr. ALTON: There seems to me to be a commercial as well as a clinical value to this question. In insurance we are getting to a point where that which is not insured has not a commercial value. Say that we have a man who is the general manager of a large corporation and who desires to secure his associates in case of his sudden demise. The question of his heart may be the one thing upon which the decision turns. He can secure his large insurance or he cannot—it may be just a question as to whether it is a functional or an organic disease of the heart. We have found that the large majority of medical examiners selected throughout the country have very little knowledge of differentiation between organic and functional disease of the heart. We often get returns from an examiner saying that he believes a murmur to be a functional murmur. We write him for his method of differentiation and oftentimes it is the lamest from a medical standpoint that could possibly be presented. That man does not know how to differentiate an organic from a functional murmur; and I am quite free to say that there are a good many others who are not medical examiners who have similar difficulties. I recall at the moment the case of a young physician employed to come to our office. I happened to be there that day myself, and he said, "Doctor, I wish you would come in and listen to this heart. Here is an organic murmur surely." I went in and put the man through the method I follow, and found that it was not an organic murmur at all but simply a functional disturbance of his heart That man was the manager of a large street railway system, and it was important that he cover himself with a large insurance. Now, going on the idea that his heart was diseased of course he would lose all possibility of insurance. I am very glad to hear of the advance in the method of differentiation in heart disease, because I think commercially it is going to be of very great value.

Dr. DARLINGTON: I never quite realized the importance of accurate diagnosis of heart disease until I became a member of the ..... Commission in New York. I then found that an enormous number of cases of death in factories were diagnosed as apoplexy. But I have

never known of a case of sudden death by apoplexy. I think we should endeavor, not only in the army but in industries, to make better diagnoses of heart disease. The question of recruits comes close to a good many of us who expect to do work of that kind—but I think sometimes we miss the point entirely. There are many, many cases of heart murmur where people do a day's work and go on in life just the same. I don't think we ought to reject every man for a heart murmur The only question is in sending men to the Border. The particular trouble there was that it was very hot and all the blood in the body was taken to the surface. That was why the man dropped in the sun—the constant heat and muscular exercise taking all the blood to the left heart and lung. I think it makes a great deal of difference where your man is going to be sent, whether to a hot country like Mexico, or to some country where it is not hot. I think all this has to be taken into consideration in our prognosis. I happen to have seen a number of cases due entirely to psychic effects—to fright, etc., and of course we see a great many in the dental clinics due to septicæmia.

We speak of the heart as the seat of the affections, and of course it does become very greatly dilated under certain conditions, but personally I think the liver is the seat of the affections One of my friends commenting on this said that of course such an idea in poetry would spoil much of the romance. For instance if instead of "Maid of Athens, ere we part, give, oh give me back my heart!" we should say "Maid of Athens, ere we sever, give, oh give me back my liver!"

But really we should pay far more attention to the heart in our cases, and the question of prognosis and diagnosis is a very important one, especially so far as soldiers are concerned, but I think we should know where men are going to be sent, their mental condition and so on. Some years ago I had the privilege of reading a paper on the rise in heart disease. Since 1908 there has been a steady rise, due partly to bad diagnosis and more probably to a more strenuous life.

Dr. SCHAUFFLER: Some of the speakers have said that a good many men would be rejected from the Army because of the confusion of functional and organic disease. We must not forget the other side of the question. The medical authorities of the Army have gone into this matter very carefully and their rule is a strenuous one, that wherever there is a suspicion of disease, functional or organic, the recruit is rejected. I think that until everyone is better educated in methods of diagnosis that is the only safe rule. Last summer I was responsible for the examination of 4,500 officers and men, and I assure you that anyone who has had a similar responsibility must realize the fact that when masses of men have to be examined in a hurry, many slip through who should not have been let through. But it is

far better to reject a man in whose case there is any doubt than to run the risk of letting him serve and break down for all the rest of his life Where it is a question of taking a man into service in times of peace and watching him for a few months or a year, I would say that the risk should be taken. But where every man on the firing line is a factor, and where it is a question of the success or failure of any movement depending upon the number of available men, we cannot take any chances at all. So that I think the rule of rejecting men where there is even a doubtful functional murmur is a pretty safe one. And we must remember furthermore that there are very few good diagnosticians available for the enormous amount of examining that has got to be done. The men who have recently been taken into the Medical Reserve Corps do not really understand army methods and army life, and too the man who in private life looks on a functional disturbance of the heart as something that with care and good diet and rest and proper surroundings can be overcome and rectified will forget that the recruit is not going where he will have good care and proper diet and rest, but is going where he will be regarded as one of an enormous number of men in a machine; where he has to do what he is told to do. If he wakes up in the morning and has had an attack of indigestion that puts his heart a little wrong he has to shoulder his pack and gun and march his number of miles whether or not. And therefore I think that we have got to be careful no longer to think ourselves in private civil practice, but to look at the thing from the standpoint of utility.

Dr. Mercur: I think we are under great obligation to Dr. Johnston for bringing up the question of organic and functional heart murmurs; and the difference between selecting a man for life insurance and one to go on the firing line has been well brought out by the last speaker. Not long ago an article was published in the *British Medical Journal* by Lewis, who worked so long with McKenzie. In a number of men invalided home with heart trouble Lewis found that in 40 per cent. the murmurs were organic the other 60 per cent. being functional. With the idea of treating them and getting the men back on the firing line he grouped those who had no demonstrable organic heart trouble all as irritable hearts and devised a very interesting series of exercises which he put the men through. His method of working it out was very interesting. He said that what caused the men on the firing line to get an irritable heart was that they could not have a motor discharge—that is, if a man ordinarily gets excited he moves around and does something and has a motor discharge. But the poor devils in the trenches can't move around. They must stay right there, entirely suppressed, and all the nervous excitement reacts back on the heart. The cure, according to Lewis, is to reverse the process by

which they could have a motor discharge. He takes pupil for pupil and gives them muscular exercises, increasing the exercises, and it was perfectly remarkable how many men who had a bradycardia and a tachycardia were by physical exercises alone enabled to go back on the firing line.

Dr. ALSEVER: This subject has been a very important one and promises to become much more so. Perhaps we ought to remind ourselves that murmurs are not diagnoses but physical signs. The last speaker brought this out well in telling us of Dr. Lewis's work, of how he found out why the soldiers had heart murmurs and then treated the cause. We must find the cause before we can say what the patient is justified in doing. This is closely allied to the discussions earlier in the afternoon regarding tuberculosis, when someone said that the tuberculosis expert was in danger of asking only, has the patient tuberculosis or not? I believe we should not stop with determining whether we have a functional or an organic murmur, but that we should consider what is the cause of the abnormal sounds in the patient's chest.

Dr. ELLIOTT: I want to say a word in regard to the use of the sphygmomanometer that Dr. Johnston has mentioned in his paper. Those of us who see a great many patients in the ward and in life insurance practice have come to find out that a pressure of 150 to 160 if constant does not have a great deal of effect upon the man's longevity or his ability to work, if he is living a fairly even life. But I am beginning to feel that the important observation from the use of the sphygmomanometer is not the actual pressure, but to find those cases where all the beats come through say at 135 while at 145 a few come through, and an occasional one is felt at 155 or over. This gives us valuable information as to the relative strength of the myocardial contractions which we could not estimate by palpation alone.

Dr. MINOR: I am sure we should all like to hear Dr. Jacobi on this subject.

Dr. DALAND: I remember that Dr. Jacobi reported an interesting case of regurgitant murmur in a child who later showed no murmur whatever.

Dr. JACOBI: Mr. President, I rise out of respect to the chair, not because I believe I can add anything that is worth listening to. I am not a military officer and so I am almost out of place here. We hear a great deal now about people on the firing line and so on and so on. I am very much interested in them, but I look on the whole process, the whole condition, from no particular viewpoint at all.

I shall speak at present of murmurs as I see them. I see them in people very old, in people of medium age and in children. They are things which should be considered only from the point of view of the patients as we see them in practice. Most of them are viewed from a different point altogether. I will give you an instance. I have seen two cases of murmur this morning in my office—loud systolic murmurs with no dilatation. One of these patients was a man 36 years old who had no change in his heart except the murmur—a very slight murmur. I told him to do some exercising, I made him shake his arms for two minutes and then stand still. There was now a very loud murmur resulting from a little bit of exertion, without any change again in the heart. Why was it that this man had this murmur? I will tell you why. His heart was very weak, and was weak for a good reason. He was 36 years old. His history I found out in a short time. He had commenced masturbating when he was 14 and continued masturbating year after year. When he was 16 he began to run after women. For two years more he gave up women but continued his masturbation—several times a week. He married two years ago, but he did not discontinue his masturbation altogether —that was not in him. Now here was a man with a fair-sized heart, no dilatation, but the heart was too weak for the exertions it had to undergo. When I saw him first this morning he had very little murmur and two or three minutes afterwards he had a very extensive murmur, which was due simply to the exhaustion of his heart muscle. I told him he could be cured if he had any common sense—if he would give up for good his masturbation, which he promised to do. I told him he would then lose his heart murmur very probably in 6 months or a year; but I had to tell him that he ought to leave his wife alone, that he must not masturbate and that he ought not to do any hard work which he was not called upon to do.

Now, Mr. President, there is a man with no business to have a heart disease—he has a feeble heart and that is his own doing. If that man gives up his past practices, in six months he will lose all his functional murmur—certainly in a year. The man can well afford to do this. He is only 36 years old, and if he improves the condition of his heart he will have a number of years ahead of him in which he can do some work. There is a functional murmur—there is nothing organic about it. I have seen a great many such cases Girls of 14, 15 and 16 will have such heart murmurs and in a few years get over them. There was another man in my office this morning in a similar condition, with no heart dilatation—a different thing and yet the same thing. This man was 46 years old, with a wife of 33. He had been married before and had no children by his first wife. He had married this wife 10 years ago and had a boy of 9, and had had no children since. His wife says that for 6 years he was never able to have

intercourse. I had to tell him that if he would send his wife away for at least 4 months and leave other women alone very probably his heart would improve. That man is well enough for all practical purposes, and will be well enough for sexual intercourse if he will wait a little while.

These hearts are not organically sick. If there is any organic disease about them it is simply a weakness of the heart muscle, and that is always improved. What I have said here appears different from what has been said by the other gentlemen. I believe you all have seen such cases. Those of you who are in general practice will see them if you ask every man, or so-called man, about his sexual habits.

That young man of whom I spoke ought to be on the firing line. I think it would be a good thing.

Dr. MARVEL: The discussion of this subject has been indeed very interesting, because it has brought out not only old points but many new ones. One point that has been emphasized and which is among the most important is the fact that we are beginning to study heart disease not from the standpoint of the lesion as it is classified by murmur and location, but from the standpoint of the lesion as classified according to the cause. And it seems to me that from this standpoint we are much better prepared to advise and direct the future of these patients than we could by merely classifying and localizing the lesion. As long as the heart compensates well under strain we know we have a reasonable resistance, and that resistance may be obtained by testing the patient in many ways. I am quite sure that those who have discussed the question here have tested patients, both men and women, in many ways—patients with very loud blowing murmurs, which if they had decided from the standpoint of the murmur itself would have led them to say that patient was really in a very dangerous condition. But after testing out the case and finding that the heart behaves very well under various forms of tests we are reasonably sure of that patient's resistance, and that his future will be very different from what it would have been if we had studied the case wholly from the standpoint of the murmur.

Dr. DARLINGTON: I disagree almost entirely with most of those who have discussed this paper. Perhaps you can get my point clearly from this: When I first came before the Climatological meetings you could not count my pulse—I remember asking Dr. Walker to put his hand on it, and he said it was zero—and now, Mr. Chairman, it does not vary one bit.

Dr. DALAND: I was interested particularly in hearing the military members of our society speak of their duties, especially in view of a

conversation which took place only two days ago in which a naval officer said "What we are concerned about is not so much any refinements of diagnosis, but we have got to have our young medical officer able to recognize a murmur of moderate intensity." This is the real question before most of the men today. I believe that one of the stumbling-blocks in discussing this question is the use of the words "organic" and "functional," and I think we ought to revise them. The murmur is not itself anything but a sign. The murmur, as I see it, should not be described as organic or as functional, but as a murmur. As to whether or not the murmur is functional, in the case reported by Dr. Jacobi, where the murmur was very slight, was that organic or functional? The point that seems to me to be clearest is that you have got to attach to the murmur the cause that produces it. It seems to be some physical condition, that may in its turn be due to a variety of things—often a stenosis may give you an endocarditis and myocardial degeneration, and that in its turn may give an aortic insufficiency. But here we have two conditions producing murmurs, each entirely different. The majority of men I have talked with I think are viewing it in this way. Given an endocarditis and a murmur, and the murmur is organic. What their exact views are with reference to a myocardial degeneration with a murmur I don't quite know. Of course the extra-cardiac crises have murmurs which may be called pseudo-cardiac. Under that head one thing has helped me a great deal; the simple recognition of the point brought out by Dr. Jacobi—namely, that the muscle of the heart is very much like any other muscle of the body, and if the relaxation of the muscle is enough to bring about a dilatation sufficient to produce a murmur that murmur is due to that particular cause; and it seems to me that most of the group of functional murmurs fall into the category of mitral insufficiency in association with dilatation and the majority of these cases come under the head either of dilatation or relaxation of the muscle.

Another point that has been very helpful is—given a heart where the first sound is insufficiently impaired and there is a questionable murmur—putting the patient through the Trendelenburg motions—touching the floor with the tips of the fingers. Watch how the patient does it. I have been able in the doubtful cases to convert a suspicious first sound into a well-recognizable systolic apex murmur, and I have had a few cases in which the balance is just on the edge (?). This procedure can be adopted by anyone anywhere.

Dr. Minor: I want to ask in this connection what are functional conditions? Those in which we can't find a physical change? There always must be a physical change behind these things. As Dr. Jacobi has said, there is a change in the heart muscle. And I do believe that it would be dangerous for the army to accept men with a functional

murmur, for they are not the kind of men who can go through the training at Plattsburg and other places. That man has got to have a perfectly good heart; and I claim that a heart with a functional murmur is not a normal heart. It is not a diseased heart—it is not an endocarditis or a myocarditis—but it is not a normal heart. I think the only attitude to take is that a man with a heart murmur is not fit to go into the army.

Dr. JOHNSTON: I want to ask the men present if any of them can give me any light on the zero diastolic pressure. In an aortic regurgitation in taking the diastolic pressure I have had it drop down to zero. Do you get this in a perfectly normal heart, or what does it mean?

Dr. HALL: Within the past two weeks I have seen that phenomenon in two patients. If you put the sphygmomanometer on the usual place on the artery I think in certain cases there is some unusual position of the bone underneath, so that the sphygmomanometer happens to touch the artery in an abnormal way, and in that case you get the murmur clear down, whereas if you move the sphygmomanometer an inch up the arm you start in the usual way.

Dr. JOHNSTON (closing): I should like to comment on one point that Dr. Darlington brought out. I don't think we know what the cause of hypertrophy is. I believe it can be mechanical. The old theory of blood damming back and causing a simply hypertrophy does not hold in every instance. The electrocardiogram shows that. We have all seen patients with large hearts and loud murmurs who have lived for many years. For a good many years I was physician for a Masonic home, with one hundred old people in my care, half of them over 70 years of age, and I heard there some of the worst murmurs I have ever heard. Those hearts were not diseased. They may have been diseased some time in the past and have recovered. On the other hand, there are cases of hypertrophy due to infection, and those are the ones in which you get a myocardial condition, a chronic inflammatory condition. Those are the ones that make trouble, and a murmur in connection with such a heart is a very different thing from a murmur in another individual. If every man in this room was to go up and down two flights of stairs we should hear murmurs in half of them. I think that is quite within the bounds of a rational statement. Those are all functional murmurs, and yet according to Dr. Minor none of those men should be permitted to enter the army.

In regard to the point Dr. Elliott raised I think that inequality of the sounds as they come through often does mean a chronic myocarditis—but it may mean a disturbance in the innervation with a

normal muscle. It may perhaps be vasomotor and be present some weeks and absent others. I have examined men and have found beats coming through all the way from 140 to 110, and have thought they might be seriously ill, and in two or three weeks, when an indigestion had cleared up, their hearts were beating regularly and every beat would come through at 140.

DIAPHRAGMATIC BREATHING.

By IRWIN H. HANCE.

LAKEWOOD, NEW JERSEY.

The title of this paper in the opinion of the writer has a broader significance than ordinarily conveyed: the mechanics of breathing is thoroughly understood, but may we not improve upon their application by developing the power and use of the diaphragm to a greater degree than is usually done. The continuous voluntary act of breathing, from birth to dissolution, has caused us to overlook the training of this muscle by voluntary action, by which means a more powerful muscle is secured and a more perfect rhythm attained; thus the habit of bad breathing is transformed involuntarily into a good one. Further the properly trained muscle is capable at will of directing the currents of air into the lower, middle and upper portions of the lungs—nay, even to inflate one half of the chest, the other remaining passive.

Respiration is an involuntary act controlled by the respiratory center in the medulla; the true muscles of respiration (diaphragm, serratus posticus superior, and the external intercostals) receive their nerve supply from the spinal column—thereby imparting to them the same voluntary action under stimulus as any other voluntary muscle of the body.

Anatomists and physiologists are agreed that two groups of muscles act in ordinary and forced respiration, the internal true muscles of respiration and the external auxiliary muscles, overaction of the latter group being always called into play in dyspnœa. In infancy and early childhood when no mental influence or habit has perverted the normal rhythm and muscular act of breathing, the internal muscles of respiration perform the

completed cycle; this is also to be noted during complete surgical anæsthesia. From six to eight years of age the child begins to lose this form of breathing and in adult life the lack of voluntary control of the diaphragm is generally noted. Ask a man to voluntarily contract his diaphragm as he would his biceps, scarcely one in a hundred can do it without contracting many of the other external muscles.

Respiratory rhythm is the measured motion of the diaphragm and the other internal muscles of respiration evoked through the stimuli transmitted from the medulla and the sympathetic, thereby producing a shallow or deep inspiration and a passive or forced respiration. What is its nature? When the diaphragm is thoroughly developed and trained to respond to the stimuli from the cerebral center, this rhythmic motion adapts itself to the occasion, whether walking slowly or fast, running, or when in repose, thereby conserving unnecessary expenditure of energy and minimizing the strain placed upon the other organs. For perfect rhythm in walking, the respiratory muscles should be trained to act in the proper rhythm, thereafter they involuntarily perform the act easily. Does not this explain what is spoken of as "second wind" in walking or climbing? It is nature's insistent call for help; as usual she responds well but slowly in combating the irregular action of untrained muscles.

Here is the crux of the writer's thoughts; first develop and train the diaphragm and when properly instructed teach the proper rhythm of breathing for the varying demands upon the pulmonary function. By the aid of proper exercises performed during this training the general physical condition will be greatly strengthened.

All breathing is diaphragmatic in the broad sense that the function of breathing depends upon this muscle allied with the other smaller internal muscles; the nomenclature of the different types of breathing as generally used would tend to obscure this fact and a better understanding of the subject would be attained by using the terms abdominal and thoracic breathing to designate

the manner in which the lungs are filled; in the former air currents expand the lungs from the lower zone upwards, in the latter from the upper downwards, figuratively dividing the chest into upper, middle and lower zones. In practice these two divisions also tend to show the power of the diaphragm in directing the currents of air into the different sections of the lungs.

For the clearer understanding of this paper, a third term is used, diaphragmatical breathing, in which a very short beginning of abdominal breathing commences the inspiratory act, quickly followed by directing the air currents into the middle and then the upper portions of the lungs. This is the most perfect example of the full finished deep breathing.

With the knowledge that air currents can be directed at will into the different zones of the chest and after observing the almost universal faulty act of breathing when a physician asks his patient to breathe deeply, the writer, thanks to his instructor, has worked out the following method. To begin with the pupil must assume the correct straight position seated in a chair or reclining; all the muscles must be in a state of easy relaxation, particularly the naso-pharyngeal and throat muscles, since only thus can nasal resonance be produced as the air passes over the floor of the nares into the larynx. This sound serves as a guide in the future education of the pupil, for thoracic breathing gives an entirely different type of resonance. The pupil is then instructed to inspire—hold—expire with the diaphragm alone, each part of the completed act taking the same length of time, a short one while mentally counting one—two, relatively six seconds. In a short time the pupil quickly learns to appreciate the limit of his use of the diaphragm, beyond which he is straining other muscles as well. After the diaphragm is under thorough control, pumping or jerking the muscle is undertaken, just like the sudden contraction of the biceps. In breathing thus the air currents are directed first and also more forcibly into the lower half of the lungs, the upper half receiving a smaller volume of air and being much less expanded. When this, which may

be designated abdominal breathing, is well performed the second part of the training is taken up: begin as in abdominal breathing while counting one—two, then by a process of stepping up direct the currents of air from the lower to the middle and then to the upper zones of the chest until the diaphragm has contracted fully and the lungs are filled to capacity. Subsequently the pupil can be readily taught to keep the upper zone of the chest full while gently breathing in and out as in abdominal breathing.

Having mastered this, which may appropriately be called diaphragmatic breathing, the third and last part becomes easy—it is the reverse of both the other forms, as the expansion begins at the apices and the lungs are filled to their full capacity from above downwards. To accomplish it properly the major currents of air are directed through the nares upwards into the attic of the nose, the smaller volume of air being carried over the floor of the nose—in other words the entire nasal cavities are filled with the incoming air which rapidly fills the lungs from top to bottom. This is properly entitled thoracic breathing and generally emphasized by teachers above the other two. Everybody is so habituated to this form of breathing that it is very difficult for a pupil to overcome the habit of breathing with the upper chest during the earlier lessons. Men and women all think that this is the greatest illustration of deep breathing, whereas this act of inspiration is seldom completed without greater or less tension upon the external muscles except one has secured the earlier control of the diaphragm as taught in abdominal and diaphragmatic breathing.

A word in regard to the expiratory act: this is wholly passive and when completed all the muscles are in a state of complete relaxation as in the primary posture. Forced expiration brings into play the abdominal and other muscles—this must be avoided in the earlier lessons.

A natural question for you to ask is, is the excursion of the diaphragm the same in each form of breathing? The X-ray pictures show that this is the case.

What need is there for such training and the daily use of pulmonary exercises? Training and daily use strengthen the respiratory muscles, correct the habit of poor or very often bad breathing, increase the capacity of the lungs, dilate more fully all alveoli, and must necessarily improve their functions, and thereby influence favorably the functionating power of other organs. We as physicians too often overlook conditions which are neither pathological nor clearly abnormal, leaving it to the layman to devise some method by which subnormal defects are corrected, thereby improving organic functions. Recognizing this, physicians are awakening to the fact that deep breathing influences well, patients under treatment for other than pulmonary conditions particularly the neurologist and the stomach specialist.

The fact that the total minimal surface over which the blood is exposed to the alveolar air amounts to 90 square meters, about 1,000 sq. ft., the size of a room 50 × 20 ft.;* that our observation as physicians shows such a large percentage of bad breathers; that the proper development of this fundamental function of the whole body is largely ignored until by reason of pathological conditions or mal-development attention is drawn to it; and the knowledge that the above method has greatly improved some conditions bordering on the pathological and cured some asthmatics induced the writer to study its value during the past three years.

In health it is superfluous to dilate upon its effects to you, a body of physicians, save to draw attention to one fact: as an exercise it compels the pupil to concentrate his thoughts entirely upon the correct performance of the act, otherwise the breathing becomes involuntary at once; as a resultant of this pulmonary training there is a marked improvement of the power of mental concentration in everything.

In disease, however, apart from the general beneficial influence of any regular muscular exercise and the resulting better-

* Ernest H. Starling, 2d edition, 1915.

ment of the functions of all organs, there should arise a more stable CO_2 and O ratio, since deep breathing means a freer circulation of blood in the alveolar vessels and a better, more uniform interchange of gases—one might picture mentally the difference in the capillary circulation of the lungs before and after pulmonary training, the same as one can demonstrate visually the changed surface capillary circulation before and after hydrotherapy has been applied. The effect upon inflammatory exudates, extra- and intra-pulmonary, would be to hasten their absorption.

The following history of the writer's personal experience shows its influence upon the irregular action of the heart without discoverable lesion: Dr. X, æt. 50 years. Healed tubercular lesion at right apex, 25 years' standing—distinct right subclavicular depression. Nine years ago annoyed by occasional lost heart beat—grew steadily worse until it occurred regularly every 4 to 10 beats. Slight vertigo at times on taking erect position from a stooping one. Three years ago lost consciousness, would have fallen but was grabbed by a friend. Several specialists could detect no heart lesion. Pulse rate naturally rapid and influenced by excitement, exercise or worry, amounting at times to palpitation. Physical powers had steadily diminished during previous five years. Blood pressure 110 mm. Smoked at times excessively. Urine examination always negative—all organs functioning properly—diagnosis functional disturbance of the heart. Result of daily systematic exercises was noted as follows: Lengthening of interval of dropped beat until at end of five months heart action was normal. For three years no recurrence. During exercises pulse count lowered 3 to 6 beats—not accelerated by ten minutes' rhythmical breathing accompanied by muscular exercise. General condition so greatly improved that he climbed a mountain 4,800 feet high over a stiff trail without any undue fatigue and no heart strain. Finally the writer offers the following for your serious consideration—its possibilities and advantages in pulmonary diseases: Tuber-

cular involvement of the lungs is mostly met with as apical lesions. Artificial pneumo-thorax has demonstrated the value in bad cases of splinting the lungs by compression, thereby giving complete rest. Abdominal breathing similates this to a certain degree, giving relative rest to the apices and expanding the bases. Most people either in repose or on exertion use thoracic breathing, therefore may we not derive benefit to the inflamed area by securing a modified degree of rest and transferring the burden of work upon the less used portions of the lungs. The practical demonstration of this is shown in the spirometer, with which the writer can show a capacity of 150–165 inches for abdominal breathing whereas in thoracic breathing he records 220–225 inches; further a close examination of the subclavicular depression in the case mentioned above can be wholly olibterated by thoracic but only partly by abdominal breathing. How much the general physical condition would be improved would be shown by its use. The writer is of the opinion that it would aid materially.

SYMPOSIUM ON FOCAL SEPSIS.

DIAGNOSIS OF FOCAL SEPSIS.*

By J. M. ANDERS, M.D., LL.D.

That both acute and chronic systemic or localized complaints may result from a preëxisting infectious focus has long been definitely known. The significance of chronic septic foci as an essential causative factor of disease, however, was not fully appreciated until the knowledge of the profession had been enriched by the labors of Miller,[1] Rosenow,[2] Billings[3] and Hartzell.[4] Indeed, recent advances in this class of affections have caused the profession to view many diseases from a different standpoint.

We are still in great need of higher standards of knowledge of the subject of focal infection, from the bacteriologic, clinical and pathologic aspects. It follows that use is to be made of any and every source of information for diagnostic purposes. The ultimate aim should be to recognize these primary septic foci before the systemic consequences become well established.

In approaching the subject of the diagnosis of primary focal infections, it is to be recollected that in a large percentage of the systemic diseases of which they may be the apparent cause, *e. g.*, chronic nephritis, arterial fibrosis, myocardial degeneration and arthritis—other factors, as heredity, occupation, intemperance in eating and drinking, syphilis, gout and the like, may play an important etiologic rôle. When any of the latter causative agencies are operative in the particular case in hand, it is not possible to determine precisely the significance of an associated chronic infectious focus, more particularly since these may at times remain latent for a long period of time.

* Read before the American Climatological and Clinical Association, at Lakewood, N. J., on May 29, 1917.

On the other hand, it not infrequently happens that a spontaneous cure of the primary focal infection, or that its removal surgically, leads to recovery, hence the recognition of the condition is a matter of the utmost importance.

At the outset, it may not be amiss to mention some of the commoner examples of acute and chronic diseases which are caused by local septic foci. Among the former are generalized tuberculosis, rheumatic fever, endocarditis, pericarditis, myocarditis, septico-pyemia, gonorrheal arthritis, cholecystitis and appendicitis, while the chronic diseases thus produced embrace arthritis deformans, myocarditis, myocardial degeneration, arterial fibrosis, chronic nephritis, septico-pyemia and many others.

While, as is well known, the infectious foci, acute or chronic, may occur anywhere in the body, it must not be forgotten that they may be also multiple, and under these circumstances may be, at times, classified into primary and secondary, and an attempt to distinguish the former from the latter is always to be made, although this is sometimes impossible.

For example, if the lymph glands of the neck are found to be the seat of a septic focus, we may reasonably infer that the infection has passed from a primary focus elsewhere, *i. e.*, from the tonsils or teeth, to these structures; hence secondary foci occur in this situation, as a rule. The secondary foci exert an aggravating effect upon the clinical features of the condition, so that they assume diagnostic importance and should, whenever possible, be discovered.

It is also of diagnostic advantage to associate the grouping of clinical features in the particular case with chronic septic foci in certain organs. For example, the primary infectious focus in chronic arthritis deformans, acute rheumatic fever, chorea, endocarditis, gastric ulcer, appendicitis, myositis, myocarditis and glomerulonephritis is usually located in the head, in the guise of tooth-root disease (periapical abscesses), sinusitis and acute or chronic tonsillitis.

In 1887, Mantle[5] first pointed out the relation of the tonsils as a portal of entry to rheumatoid infection—an observation confirmed by the recent researches of Pointon and Paine[6] and others. In cases of suspected gonorrheal arthritis, an examination of the genito-urinary tract (posterior urethra, prostate, seminal vesicles, renal and perirenal area, pelvis of the kidney, bladder) should reveal the seat of the focus. Acute miliary tuberculosis points to chronic foci in the lungs or thoracic lymph glands.

Unfortunately, we have not as yet an accurate knowledge of all the sites of these septic foci in the body, nor of the percentage of the cases in which the disease germs gain admission to the circulation through these dangerous gateways.

Lane[7] has attempted to fix professional attention upon the abdomen-colonic infection, but as Morris[8] pertinently remarks, "We are not to concentrate our attention upon the colon in searching for focal infections. We must look over the periphery of a large circle of causes in order to find a possible focus which is sending out toxins that are being excreted at a certain point where there is a selective cell affinity for these toxins."

The internist should not neglect to examine the lungs and pleura for the primary focus in obscure septic states, more particularly if the latter are preceded by lobar pneumonia. An abscess may be found either in the lungs or one of the pleural sacs. The expert gynecologist may discover the initial lesion in the uterus, the tubes, ovaries or glands of Bartholini.

The clue given by the character of the disease, however, may fail to reveal the primary focus on painstaking, thorough investigation in the supposed location. Frank Billings,[9] whose name is inseparably associated with the subject of focal sepsis, pertinently observes: "The failure to find a focus in the expected situation should indicate an extension of the field of examination until it shall have been found."

Goadby[10] aptly states that "in many mouth infections, an exceedingly small local cause may produce suddenly generalized

and widespread infection without apparent exacerbation of the mouth-disease preceding such infection."

Says Daland:[11] "The diagnosis of mouth sepsis should be made by a specially skilled dentist, as a rule," and in this opinion I concur. There are not a few obscure cases in which a consultation between a dentist and physician is to be advised and encouraged. In this connection, Dr. E. F. Everett[12] pertinently remarks: "There remains much to be unearthed in regard to these conditions which can only be accomplished by the hearty coöperation of one profession with the other."

In suspected cases, the service of the X-ray expert is also required, but no amount of X-ray evidence alone, is sufficient for a reliable diagnosis, nor to furnish a basis for treatment. The results obtained by the Rœntgenologist must be weighed in connection with the bacteriologic findings, anamnesis and the associated symptoms. While bacterial cultures from the gingival surface will usually yield pathologic types of bacteria, they are "not an index of focal infection located in the dental alveoli or tonsils" (Billings).

In these cases and those brought under suspicion by the Rœntgenologist, the specially skilled dental surgeon should investigate into the pulp canal from apex to the extreme base. Not only so, but the periapical space should be aspirated and the withdrawn material examined microscopically and culturally.

The infected area or alveolar abscess may be entered either through the root canal or by passing over the exposed neck of the tooth involved, after removing the "tartar" and any exudate that may be present, penetrating as deeply as possible. The discovery of one of the salivarian group of streptococci, *e. g.*, the *viridans*, *hemolyticus* or *mucosus*, sets the question of the diagnosis of a septic focus at rest, but does not establish its specific relation to the general infection.

Obviously, if the pathogenic bacterium found in the outlying infected tissues do not differ from those discovered in the local focus, the etiologic connection between the local condition and general disturbances is reasonably well established.

In this connection, the studies of Hartzell and Henrici[13] are of much interest and importance; they state "that the *Streptococcus viridans* is the only organism found constantly in chronic alveolar abscesses and in the great majority is present in pure culture." They have at times found other organisms, particularly the white staphylococci and fusiform bacilli, but they believe that these are secondary invaders and practically devoid of pathogenic action.

These observers continue: "From acute abscesses we have recovered hemolytic streptococci on one or two occasions, but the majority of our cultures, from lesions where free pus occurred, have yielded staphylococci (also hemolytic at times) and the fact that these staphylococci have usually been found associated with the mouth streptococci, together with histological findings in the affected tissues, lead us to believe that many of the acute abscesses indicate a secondary invasion by staphylococci with a suppurative process implanted upon the primarily granulomatous lesion. Occasionally, purulent infections are found where other organisms are apparently the etiologic factor, such as the pneumococcus, diphtheroid bacilli, *Bacillus pyocyaneus*, *Bacillus fecalis alkaligenes*, etc.; such infections are not common, nor can they be considered characteristic dental lesions." If future observations by other bacteriologists confirm those of Hartzell and Henrici, the recognition of mouth sepsis will have received a decided impetus. The inoculation of animals with strains of the organisms mentioned above, obtained from the foci, is to be urged, since the general pathology and symptomatology of a particular septic condition may thus be elucidated. Such laboratory demonstrations have been carried out successfully in recent years in connection with a limited number of affections, more particularly acute rheumatic fever and chronic arthritis.

A diagnosis, however, is usually reached by a far easier route, by physicians, than the foregoing methods necessitate. Unhappily, many practitioners have fallen into the error of making hasty inferences based on inaccurate and superficial observation.

This fact can be amply confirmed by the experience of those who have, at the suggestion of the physician or Rœntgenologist, removed the supposed primary focus with the expectation of effecting a cure, but without permanent success.

As elsewhere stated, "attention can not be too strongly drawn to the fact that before consulting an expert dentist, in cases in which mouth sepsis is suspected, the physician in charge must, by a process of exclusion, eliminate all foci of infection other than those that may be present in the mouth.

"In these preliminary examinations, the advice of a competent nose and throat specialist is always necessary. Without observing this precaution, either before or after consultation with a skilled dentist, with a report of the results, an unfair advantage is taken of the dental profession."[14]

In cases in which the focus is located in the sinuses and tonsils, the diagnosis of the local condition must be made by a skilled and progressive nose and throat specialist, who should not omit from his studies of the particular case, a careful cultural examination of any exudate that may be obtainable from the focus. This method of investigation is to be pursued by the clinician with all thoroughness when primary foci are found upon a careful search in other portions of the body than the head, *e. g.*, the lungs, pleura or gall-bladder.

In some obscure cases of focal sepsis, the diagnosis resolves itself into a cultural study of any vaginal, uterine and urethral discharges (the latter being obtainable at times only by massage of the prostate gland and seminal vesicals) as well as of a microscopic and cultural examination of the feces, sputa, urine, pleural or synovial exudate and blood even. For example, strains of streptococci, which may invade the walls of the intestines, have been found in the intestinal flora. Furthermore, such focal infections, be they slight or extensive, may, particularly if encapsulated, produce the features of a secondary systemic infection. With these thorough laboratory studies must be combined an equally thorough, general examination, including a careful anamnesis.

I can not forego an opportunity to urge that great care and caution are required to avoid ascribing to primary focal infections, systemic conditions due to other major causes, that may be operative. Neither is the practical fact that a local focus, *e. g.*, a frontal sinusitis, may be secondary to and dependent upon a primary general infection to be overlooked. Such foci may disappear with the subsidence of the systemic features. On the other hand, they may tend to persist and thus become an aggravating cause of, and greatly prolong, the course of the general symptoms.

In concluding, the diagnosis of a primary focus embraces the discrimination of non-focal infections and the establishment of its seat and particular etiologic variety both from a clinical and a bacteriologic viewpoint, supported by reliable X-ray findings in many instances, and also expert opinions in the cases of tonsillar and sinus involvement. The fact is to be reëmphasized that X-ray studies should supplement, and not displace, other methods of diagnosis, particularly in cases of mouth sepsis. In the present state of our knowledge it would appear that the diagnosis of a primary oral focus demands the isolation of the *Streptococcus viridans*.

Lastly, the specific relation of chronic septic foci to systemic diseases does not as yet stand upon a sure and lasting foundation.

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FOCAL SEPSIS AS A CAUSE OF DISEASES OF THE JOINTS AND OF THE GENITO-URINARY SYSTEM.

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I HAVE three cases of colon bacillus infection of the kidney, which did not recover until after the removal of the appendix and, in one of them, the gall bladder too. I will report this one in full.

Mrs. S. C. Age 52. Housewife. Consulted me October 31, 1916, complaining of shortness of breath and pain in the back. Her family history is negative as having any bearing on the present condition.

Previous Personal History.—Measles, chickenpox in childhood. Whooping cough five years ago. For many years has been subject to severe headaches accompanied by nausea and vomiting. Has had none for two years. Menstruation began at 15, stopped at 41. Ten children. No miscarriages. Two years ago she began to bloat; urine diminished in amount and she developed shortness of breath, throbbing in her chest. These symptoms have continued up to the present time. At present her appetite is poor, no gastric distress, a little belching. Bowels do not move without a laxative. She sleeps well. Has occasional dull headache and is dizzy at times. She has no cough but has some morning expectoration and is short of breath.

Physical Examination.—Lungs and spine normal: heart normal in size, shape and position; first sound at apex very loud; aortic second sound markedly accentuated. No endocardial murmur. Abdomen—no enlargement of liver or spleen. No abdominal mass palpated; tenderness in epigastrium and under right costal margin over site of gall bladder—tenderness very slight in McBurney's point and posteriorly in right costo-vertebral angle. Considerable edema of legs, puffiness of ankles and flat-foot on both sides.

Pulse 100, regular and moderate volume Blood pressure, systolic 180 mm. Hg, diastolic 106 mm. Hg. Temperature 98.6. Tongue clean. Weight 137½; greatest 162, five years ago. Blood count shows a very moderate secondary anæmia, nothing else. Stomach content a moderate superacidity, no other abnormality. Stool no abnormality. Urine, single sample, specific gravity 1,018; reaction highly acid; albumin trace, indican excess. Sediment considerable, consisting of pus, round and squamous cells and several hyaline and granular casts. X-ray demonstrated a much diseased appendix, a general intestinal ptosis and an irregularity of the duodenal cap. I made the diagnosis of chronic appendicitis and probably cholecystitis and infectious nephritis.

Operation was declined, so a diet and mode of life was given her to follow.

She returned to her home and reported in January, 1917, saying that she was better, but she had lost some four pounds in weight and upon close questioning and further examination it was evident that she was no better, but was gradually losing ground. A culture of her urine showed colon bacilli. She was at last persuaded to have an operation.

On January 17, Dr. Marshall Clinton operated, a diseased appendix, post cæcal was removed, her gall bladder was drained and a large number of gall stones removed.

On April 30 her husband wrote me: "She has continued to improve since her operation, has gained 10 lbs. in weight, is feeling well and doing her own work." He sent me a sample of urine which, of course, was not sterile, so could not be cultured, but showed no casts and only a few pus cells.

L. B. Female. Age 53. W. House work. Entered Buffalo General Hospital, November 27, 1916, complaining of swollen and painful joints, knees, ankles, elbows, wrists and small joints of hands.

Family history negative.

Personal History.—Scarlet fever at 17. Typhoid fever at 24. Malaria at 30. Remembers no other diseases, excepting several attacks of rheumatism, the first one three years ago. The most severe last July. The last three weeks ago.

Menstruation began at 14, regular, often painful. Ceased at 40. No venereal disease. No laboratory evidence of syphilis or gonorrhea.

She ran no fever while she was in the hospital. Her blood pressure was 165/115.

The only abnormal conditions found on physical examination were the condition of the joints, a faint mitral systolic murmur at the apex, an accentuated aortic closure, an abdominal scar of old operation for removal of cystic ovary.

On December 12, Dr. Fairbairn reported both ethmoidal areas show evidence of infection. December 29, polypoid tissue was removed to allow free drainage.

January 10, as the expected improvement did not occur, teeth were found to be infected and four were removed from her lower jaw.

Two weeks later improvement began and was steady. She left the hospital May 18 in good shape.

L. M. Age. M. Was seen January 13, 1917, with a history of having had rheumatism for some months and of having had her tonsils removed September 24, 1916, and very slow recovery from the operation.

I saw her again May 3, 1917. At this time she was weak but slowly growing stronger; her joints, however, were still bothering her. Her temperature was 97.6—pulse 76—blood pressure 100/50. Blood count showed a very mild secondary anæmia, her urine a high specific gravity and a marked indicanuria and high acidity. Her teeth were X-rayed and infection found at the roots of four, two of which had been capped. These teeth have been removed and the patient is steadily improving.

The next is an especially interesting case as showing an arthritis from local septic focus and later nephritis from another local focus.

Mrs. A. B. H. Age. M. Consulted me October 26, complaining of arthritis. I quote from Dr. Plummer, one of our best orthopedic surgeons who studied her joints by X-ray: "As to the possible cause of these lesions, her history of chronic gastro-intestinal trouble suggested the possibility of depraved nutrition, which is a usual accompaniment of cases of so-called atrophic arthritis, not accounted for by trauma or acute infection." She had previously had a gastro-jejunostomy and a uterine fibroid removed. She was placed upon laxative and diet and alkalies. Improved slightly but did not recover until appendix was removed. Then disappeared from view.

A year or two later she developed a nephritis with some pus in urine. Unfortunately no culture of urine was made, but she had edema, anæmia and albuminuria with casts. In February, 1916, she developed an acute pansinusitis, which was operated on by one surgeon and then by another and treatment kept up by the latter. Her blood in March, 1916, showed hemoglobin 72 per cent.—erythrocytes 3,984,000. Her urine 2,000 c.c., specific gravity 1,018, a large amount of albumin, many pus cells, occasionally coarsely granular cast. In February, 1917, hemoglobin 95 per cent., red cells 4,960,000. Urine 1,500 c.c., specific gravity 1,009. No albumin, a few pus cells, occasional red cell, many calcium oxalate crystals, no casts.

A. T. Male. Age 27. American. S. Chauffeur. Entered Buffalo General Hospital December 16, 1916, complaining of pain in right shoulder and hip, both knees and both ankles; stiff neck, headache and weakness.

Family History.—Good except for rheumatism in older sister.

Personal History.—Pertussion, mumps, measles, and scarlet fever in childhood. A severe sore throat at 20 years of age. Gonorrhea at 24 years. In the early part of November had what he called grippe, with pain and aching in various parts of his body, no redness or swelling of joints, very slowly grew better. On December 11, he went downtown. The next day was taken with pain in many of his joints, gradually developed swelling and redness. He ran the course of an ordinary rheumatism of mild type, with moderate fever and increased frequency of pulse and a leucocyte count of 15,000.

Under diet, local applications and sodium salicylate the condition improved and grew worse again.

On February 5, his tonsils were found to contain pus with a streptococcus as cause.

On March 7 the tonsils were enucleated by Dr. John F. Fairbairn.

Salicylate medication was gradually reduced and stopped entirely March 27. He left the hospital April 7, having had no recurrence of the joint infection.

S. C. Male. Age 37. Russian Pole. M. Bricklayer. Entered Buffalo General Hospital March 23, 1917, complaining of general soreness and pain through body, with especial stiffness and pain in ankles and elbow.

Family History.—Negative. Wife is well, has had six children of whom four are living.

Personal History.—Ten years ago had some sickness which confined him to bed for five weeks, remembers no other sickness, has been a moderate user of alcohol and tobacco. Twelve weeks ago, this trouble began with pain in his ankles, but he kept at work until six weeks ago, when both elbows were stiff and painful on motion. X-ray showed slight evidence of infectious arthritis of these joints.

Physical examination failed to reveal any local focus, but study of his blood showed a positive, two plus, Neisser reaction and a urologic examination showed a chronic vesiculitis, from which some pus could be expressed. No gonococcus was found in this exudate, but the patient having been relieved from his joint pain by rest, diet, alkalies and sodium salicylate would not allow further study or local treatment of his vesicle.

With the evidence of chronic disease of the vesicle and the positive Neisser reaction in his blood, I have no doubt as to the etiologic relation of the local focus in his vesicle to the arthritis.

FOCAL SEPSIS AS A CAUSE OF DISEASES OF THE RESPIRATORY AND GASTRO-INTESTINAL SYSTEMS.

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When we consider how universal is mouth infection and the frequency of demonstrable lesions of the tissues of the mouth in persons who are, clinically, in apparent good health we may be pardoned if we fail to accept without question the results

Fig. 1. Dissection of the mandible, showing the intimate relation of the teeth to the cancellous tissue with the opportunity for spread of infection.

which are claimed of the wonderful cures by treatment of these tissues. The same may be said of other foci.

We may well inquire whether or not on account of focal infection, teeth are being needlessly sacrificed and tonsils slaughtered; appendices being removed and gall bladders drained

Fig. 2. Lateral view of a lower tooth showing the cancellous tissue. There had been some resorption of the margin of the bone but the remaining bone is apparently in a healthy condition.

unnecessarily; genito-urinary organs being operated without sufficient cause; portions of the gastro-intestinal tract being removed or short circuited unjustifiably? We may safely answer yes.

However, in the light of accumulated evidence of competent and trustworthy witnesses, if we were asked if many of these cases which might be benefited by judicious surgical interference or other appropriate treatment are not being neglected, we would also reply in the affirmative.

While some have become over-enthusiastic, others have been ultra-conservative. Fads find their place in medicine. Time is required to form a just estimate when there exists apparent contradictions. Our knowledge of focal infections is in a chaotic state.

Infective organisms may remain in tissues for years without

producing appreciable symptoms, either focal or systemic, and later through some exciting cause, produce acute disease, or they may produce chronic focal and systemic manifestations; or

FIG. 3. Photomicrograph showing from left to right the tooth, the peridental membrane and the alveolar process with some of the cancellous tissue near the margin of the alveolar process.

produce systemic conditions when the focal manifestations may be obscure.

Gastric ulcer and other infectious conditions of the gastro-intestinal tract have been attributed to infection from foci in the

mouth. The following resumé of a case is illustrative of an obscure condition of gastro-intestinal disease in which relief

FIG. 4. Photomicrograph illustrating a cross-section through the periapical tissue showing the peridental membrane and the cancellous tissue. It illustrates how readily infection which involves the peridental membrane may spread to the cancellous tissue of the maxillary bone.

followed removal of infected foci: Male, aged forty-two years. Declining health for several years. Unable to work since July, 1915. Gave history of having had pain in region of gall bladder

and of vomiting bile. Greatly emaciated. Had been variously diagnosed chronic gastritis, cholecystitis, tabetic crisis, carcinoma

FIG. 5. The gingiva attached to the alveolar process, falling in festoons over the necks of the teeth, forming a curtain-like extension of the gum tissue folding back on itself, detached from the portion of the teeth which it covers. It is composed of epithelium and a connective tissue sub-layer. The epithelium consists of a horny layer which externally is resistant, becoming less pronounced as the tissue folds back against the teeth. The connective tissue layer is directly continuous with the periosteum at the margin of the alveolar process and the pericementum. It is composed of closely fitted bundles of fibrous tissue beset with numerous papillæ. It is against the gingiva and the teeth that salivary and cerumal calculi are formed which produce irritation, and between the gingiva and the teeth that serumal calculi frequently produce the lesion for infection. Food may crowd up under this and undergo decomposition, or tissues may be wounded, frequently are by the use of the toothpick, etc., starting a focus of infection.

and gastric ulcer. Entered Los Angeles county hospital July 8, 1915. Wassermann negative. Fluoroscope failed to reveal ab-

normal conditions of abdominal viscera. Exploratory laparotomy. Gall bladder drained. No improvement. After several months a number of decayed teeth and roots removed. Improvement rapid. Discharged March 3, 1917.

FIG. 6. Photomicrograph showing a section of a healthy gingival tissue with its epithelial fold and papillæ.

Three cases of chronic empyema, tuberculous patients each of whom had had rib resections, entered the Los Angeles county hospital within a few months. Each suffered from pyorrhœa. Two of the cases ceased to drain following treatment of mouth conditions. One was apparently uninfluenced. Attention to

the mouths of the above cases was given by Dr. Barrows in charge of the dental department of the hospital.

FIG. 7. Photomicrograph showing papilla and an early inflammatory process taking place in the tissues of the gingival fold.

I might multiply cases of success and failure which have come under my observation following treatment of focal infection of the mouth and other portions of the body but I think it unnecessary.

(I discussed briefly Focal Infections in Relation to the Etiology and Treatment of Tuberculosis in a paper read before the Southern California Dental Association, published in the *Pacific Dental Gazette*, September, 1916, copies of which were sent to the members of this Society.)

During the past few years so much has been written confirming the beneficial results obtained as the result of treatment of diseased foci on symptoms and pathological processes in other portions of the body, that cases which I might report would be duplicating.

Sufficient evidence is at hand; therefore, I think we must accept the influence of focal infections in production of diseased conditions of the thoracic viscera and the gastro-intestinal tract, also the influence of these infections on pathological processes of these viscera.

FIG. 8. Photomicrograph showing to the right a section of a tooth with a fracture of the pericementum which occurred in making the section. The white or clear space shows where the fragment has been separated from the tooth. In the lower part of this separated fragment is seen the attachment of the gingival fold to the pericementum. Its tissue shows inflammatory foci.

The finding of a diseased focus and its removal does not always prove therapeutically successful, but it has removed one stress to which the individual was being subjected. Other foci may exist and the one removed may not have been the one chiefly concerned in the production of symptoms. All foci should receive treatment in so far as they may be discovered

and it is compatible with the best interest of the patient. Disappointments will continue to occur with this as with all other measures of treatment until we are able to appreciate and successfully care for all factors entering into diseased conditions.

Fig. 9.

We have reached the period in this line of work when I believe the improvement of prophylaxis should receive attention, and we should attempt to discover early those conditions which, if neglected, will bring about chronically infected foci. For this reason I am bringing to your attention today, very briefly the condition of one group of tissues which frequently is the seat of

focal infection, that of the teeth and adjacent tissues. I desire to consider these from the standpoint of histology and some of the anatomical relations which favor pathological conditions.

FIG. 10. One of the extreme later results of infection of the peridental membrane with which you are all familiar. It is an extra-osseous infection.

I am indebted to Dr. Julio Endleman, professor of special pathology, etc., in the University of Southern California College of Dentistry, for the use of many of the illustrations herewith presented. They will appear in a forthcoming work by Dr. Endelman, "General and Dental Pathology for Dental Students."

Fig. 9 (photomicrograph) illustrates chronic inflammation of the peridental membrane, showing the separation of the peridental membrane from the tooth, also the limiting of the inflammatory process. This suggests how extensive lesions may take place without producing discomfort, the process being limited in extent, chronic and the drainage free. It also illustrates why extreme care should be used in cleaning the teeth in order to reduce the mechanical injury to the minimum, thus preventing further extension of the diseased process. By rough use of instruments the mechanical barriers established for limiting

the extension of the infection may not only be broken down, but infectious material may be carried into uninfected areas. Many cases manifest symptoms of sepsis following scaling of the teeth. The area treated should in many cases be limited to a few teeth at one sitting. Even by careful manipulation, the

FIG. 11. The resorption of bone caused by an extra-osseous infection. Observe that the root canals are well filled and that no intra-osseous infection is revealed by the X-ray. Note the overhanging crowns with the marked resorption of alveolar process. These ill-fitting crowns favor infection. It is almost allowable to paraphrase in expressing the condition of the tooth wearing the shell crown by stating that infected rests the tooth that wears the crown.

lymph spaces are opened and conditions favorable to septic absorption produced. I had one case in whom aching of the joints always followed scaling of even one tooth, which would continue for two or three days accompanied by "hay fever" symptoms. The scaling of three or four teeth would confine her to her bed with slight rise of temperature and more manipulation would be followed by more intense symptoms. Her teeth were finally extracted. This was followed by gradual but marked relief of symptoms.

Fig. 12 illustrates a pronounced, moderately early indication in the upper gingiva of the processes which result from the pathological conditions which have been illustrated by the foregoing, while in the lower gingival tissue a more advanced stage is seen. The persistently slightly swollen red line so frequently

Fig. 12.

seen at the margin of the gingiva is the signal of infection at a time when the tissues are amenable to treatment. It is the beginning of a process which usually is painless and does not produce symptoms for many months, and frequently not for years. It is during this early manifestation that the tissue should be treated. I am convinced that few physicians or dentists appreciate this, or recognize the conditions as early as they should. We not infrequently see patients who have pronounced lesion of the gum tissue with recession, who when their attention is called to the condition say they visit the dentist regularly every six months, or that they have just had a large amount of work completed, and the dentist has neither treated nor called their attention to this condition. Waiting until pus can be expressed from the gums bears the same relation to these conditions that waiting until the finding of tubercle bacilli in the sputum does to the early diagnosis of tuberculosis. Early recognition and early treatment are needed. The X-ray is not an aid in diagnosis of early extra-osseous infections.

I wish to call your attention to the limitations of the X-ray in the diagnosis of peridental intra-osseous conditions. Fig. 14 is one of several X-rays taken of this patient. No pathological conditions could be recognized by the X-ray, yet continued tenderness caused the patient to urge the extraction of the

FIG. 13. The intra-osseous infection shown by the X-ray, with which all are familiar. In this instance the pathological condition is probably due to a carious tooth having been crowned, the pulp having died and infection extended through the root canal.

tooth after a year or more of treatment. When the tooth was extracted resorption of the apex of the palatal root was found to have occurred. Marked amelioration of painful affection of the right knee joint which had existed for a year was experienced. Relief has continued since—for a period of about six months.

SUMMARY.

Our knowledge of focal infection is still in a chaotic state.

Focal infections are frequent without being accompanied by active clinical manifestations.

Focal infections are productive of systemic manifestations of disease, are the cause of diseased conditions in remote regions of the body and influence unfavorably the progress of other diseased conditions existing in the body.

FIG. 14.

Foci of infection due to the same or different infective organisms may coëxist in different tissues of the body.

Care should be exercised in disturbance of extensive foci of infection. Mechanical tissue barriers enter into the protection of the body against the injurious influence of infective organisms and their toxins. These also probably act as a protection of the infective organisms against the efforts of the host to rid itself of them.

The virulence of different strains of infective organisms varies, as does also the resisting powers of the hosts. The effect of an infection depends on relative virulence and powers of resistance.

The removal of focus of infection, in such a manner as not to throw too great additional immediate stress on the body,

may remove a stress which will have a favorable influence on a diseased condition not due to the focal infection.

The X-ray is a valuable adjunct in recognizing these conditions, but can not be relied on implicitly either when the findings are apparently positive or negative—it is a link in the chain of evidence. Many cases of chronic focal infection may be prevented if the infection is recognized early. In this paper I have endeavored to call attention especially to a very common one which is easily observed and responds readily to treatment—gingivitis.

FOCAL INFECTIONS AS A CAUSE OF CARDIOVASCULAR DISEASE.

By ROBERT H. BABCOCK, M.D., LL.D.,
CHICAGO.

So much has been written on the rôle played by focal infections in the causation of systemic and visceral disease that another contribution to the subject seems like a work of supererogation. Moreover as I cannot add anything new I can only hope to emphasize some of the facts already established by laboratory investigators and by clinicians. This will be true particularly of the work of E. C. Rosenow, formerly of Chicago, but now of the Mayo Foundation. His investigations have resulted in two discoveries of fundamental importance in the understanding of our subject; namely, the transmutation of the several strains of the streptococcus by proper cultural methods and by animal passage, and secondly the selective affinity displayed by streptococci for special organs and tissues.

Both these facts have an intimate bearing on the relationship of focal infections to visceral and therefore cardiovascular disease, but in particular it is the latter, or the property of a streptococcus to attack certain structures, which throws light on the etiology of the diseases considered in this paper.

Although the *Streptococcus hæmolyticus* and the pneumococcus, as well as some other organisms not belonging to this group, may and do attack the heart, it is the *Streptococcus rheumaticus* and the *Streptococcus viridans* that display special predilection for the cardiac structures. As is now generally recognized, it is a throat infection and usually a tonsillitis which is the infective focus from which the germ of rheumatism gains entrance to the blood and thus is carried to the heart valves, pericardium

or myocardium, as the case may be. But this is not all we need to bear in mind with regard to the faucial tonsil as a focus of infection. We should remember that a chronically infected tonsil may harbor streptococci for a long time and be the focus responsible for repeated attacks of inflammatory rheumatism with their evil consequence for the heart and blood vessels.

This liability to recurring attacks of rheumatism residing in chronic tonsilar infection is bad enough, but a still worse possibility confronts the cardiopath who harbors this focus of infection. Either because in such diseased tonsils are provided cultural conditions favoring the transmutation of the hæmolytic or rheumatic strain of streptococcus into the *Streptococcus viridans* or because this latter organism gaining access to the tonsilar crypts finds therein, under decreased oxygen pressure, conditions suitable for its growth, abundant evidence has been obtained to prove that septic tonsils are a frequent habitat of this dangerous strain of streptococcus. Consequently given the combination of a chronic valve lesion and an infected tonsil, we have precisely the conditions essential for the development of malignant endocarditis. And in this connection let it be stated that more than once in my experience has a portion of tonsil left after an incomplete tonsilectomy been found to be the starting place of fresh attacks of acute arthritis and even of cardiac involvement. In fact such an instance is in the hospital at this present writing.

It may be urged by opponents of the dictum that all infected tonsils should be removed that their enucleation does not guarantee against a future attack of rheumatism or endocarditis. Doubtless such is the case, but ample justification for the operation is found in the marked improvement in the patient's general health and in the freedom from rheumatic symptoms that had caused previous annoyance and anxiety. Moreover the objection that may be urged against operative interference with tonsils, which do not look very bad yet have been the seat of repeated inflammation, may be met by the fact that such tonsils often are found to contain pus and serves only to emphasize the

importance, nay, necessity of prompt attention to every attack of sore throat or so-called grippe and of putting and keeping in the best possible condition all parts of the upper respiratory tract of children and adults who have once suffered from arthritis or involvement of the cardiac structures.

Of other infective foci none are more serious as regards the cardiovascular system than those of the mouth, pyorrhœa alveolaris and abscesses about the teeth roots. The studies of Gilmer and Moody at St. Luke's Hospital, Chicago, not to mention other workers, have demonstrated conclusively that the organism commonly found in these foci is the *Streptococcus viridans*. Not only is many a case of fatal endocarditis caused by this germ, but as shown by Rosenow it displays a selective affinity for muscle and accordingly for the myocardium. A case has already been reported by me of a young man whose chest pain and cardiac incompetence has led to the diagnosis of coronary sclerosis. Three Wassermanns as well as history of luetic infection were negative and so likewise was a history of articular rheumatism, but there was discovered an alveolar abscess from which was obtained by culture the *Streptococcus viridans*. Upon extraction of the diseased teeth and the cleaning out of the abscess cavity followed by appropriate treatment by means of an autogenous vaccine, rest in bed, digitalis, etc., the heart regained its functional efficiency and the chest pain so far disappeared as to permit pedestrian exercise previously impossible. The conclusion in this case seemed warranted that it was the alveolar infection by this particular organism which had led to a parenchymatous myocarditis and consequent cardiac incompetence, also that the pain was due to myositis of the tender pectoral muscles.

Before dismissing this portion of our subject I desire to direct attention to the frequency in my experience of the association of chronic tonsillitis and suppurative processes in the gums and about the teeth roots. This may not be the case in children but is so common in adults that the detection of infected tonsils

renders us very suspicious with regard to the teeth and leads to careful investigation of the latter, often by means of X-ray films, without which the existence of alveolar abscesses oftentimes cannot be recognized. And in concluding this part of my theme I must register the emphatic opinion that abscesses of the jaw, and often pyorrhœa, cannot be cured by any other means than by extraction of the diseased teeth.

The structures already mentioned are not the only situations in which may lurk the germs dangerous to the heart. In 1909 I reported cases of cardiac incompetence and arrhythmia associated with, and apparently dependent upon, chronic cholecystitis. A satisfactory explanation of this connection was not forthcoming at that time, but since then Rosenow's investigations regarding the bacteriology of gall-bladder infection afford a very reasonable explanation of the association of myocardial incompetence and cholecystitis. As we all know he has identified the streptococcus as the etiological agent in practically all cases of cholecystitis, and this fact taken in connection with the known transmutation of streptococci in the animal body and the frequent recognition of lesions in the heart muscle of rabbits after intravenous injection of streptococci, renders highly probable the theory that cholecystitis, either chronic or acute, may prove a septic focus from which streptococci gain access to the myocardium. Functional disturbance of the heart in the form of arrhythmia may of course result from hepatic colic or gastric disorder, but the conclusion appears justifiable that long-standing gall-bladder infection may be responsible for the cardiovascular changes not infrequently found in persons with chronic cholecystitis with or without gall stones.

Laboratory investigations and animal experiments render the assumption reasonable that other foci, as appendix, pyosalpynx, etc., may be the starting point for acute or chronic disease of the cardiovascular mechanism and clinical evidence of this view is not wanting. In this connection one must not omit consideration of the intestinal canal as a focus from which, as Rosenow

has shown, streptococci may enter the circulation and set up changes in the heart, blood vessels or kidneys. And with reference to these latter organs we may remind ourselves that acute and chronic nephritis apparently attributable to infection of pharyngeal tonsils and teeth has been reported by Billings and has been cured or ameliorated by removal of the diseased foci.

A fruitful field of speculation and research is the influence of focal infections on the endocrine glands and special interest attaches therefore to the relation existing between such foci and perverted function of the thyroid. It is still too radical to assert that all cases of hyperthyroidism are due to focal sepsis, but reports by Billings and associated workers at the Presbyterian Hospital in Chicago as well as observations of my own have convinced me that an etiological connection does exist in some cases between Graves' disease and infection of the teeth or pharyngeal tonsils. At all events practitioners would do well to bear this possibility in mind in treating the cardiovascular disturbance of hyperthyroidism. As it seems to me the physician is not doing his full duty in the management of any chronic infection unless he endeavors to search out the possible etiological factor or factors and on discovering infective foci advises their removal.

It is not necessary to dwell on the liability of acute focal infections to cause septicæmia and endocarditis or even acute interstitial myocarditis, for the seriousness of abscesses and infected wounds was recognized long before the germ theory of disease rendered clear the relation of the primary lesion to the systemic and visceral disorders.

So illuminating have been the facts developed in research laboratories regarding the pathogenic influence of focal infections that it is not preposterous to assume that in the near future additional data will be furnished to explain changes in the cardiovascular apparatus that we have been wont to consider degenerative and due to senility or obscure metabolic disturbances.

Take for instance arteriosclerosis and the condition now known by the term hypertensive cardiovascular disease. It is not unlikely that the sclerosis of the vessel coats may date back to infectious processes which have been thought well rid of because there are no longer active symptoms, yet which may have left behind some hidden or neglected focus harboring streptococci or other bacteria hostile to the various parts of the circulatory mechanism. If Crile's theory regarding the effect of infections either local or general on the so-called kinetic system is correct (and I must confess that it appeals strongly to me) then many cases of arterial hypertension, of which the etiology is now obscure, may be due to the influence of focal sepsis on the adrenals. Certain it is that many an individual suffering from too high blood pressure is found to have septic processes in throat, mouth, gall bladder, appendix, etc., which may be acting injuriously on the adrenals and cardiovascular system, although their removal by operation may not restore the individual to a healthy state. At all events, I am frank to confess that with increasing experience I am more and more inclined to believe that eventually all maladies now of obscure etiology will be found to be of bacterial origin.

To revert to the intestinal tract and to cases of arterial hypertension, it is interesting if not important in our endeavor to ascertain its etiology, to call attention to the action of the *Bacillus aminophylus intestinalis* on one of the amino acids formed in the digestion of protein. The amino acid in question is tyrosin and it has been shown that when this amino acid reaches the cæcum it may be attacked by the *Bacillus aminophylus intestinalis* and be converted into tyramin, which latter substance possesses the property of contracting blood vessels and raising blood pressure, being in this respect one fourteenth as powerful as the secretion of the adrenals. It is quite possible therefore that the injurious effect of an over-consumption of meats and other proteins may account for the high blood pressure found in some individuals in whose anamnesis no definite etiological factor can be ascertained.

In this connection it is also of interest to mention that histamin, a toxic substance produced by the action of the aminophylus bacillus, in the cæcum on the amino acid, histidin quite contrary to tyramin relaxes the blood vessels and lowers blood pressure. May not this explain those cases in which tachycardia and irritability of heart action are found associated with arterial hypotension and no definite signs of organic cardiac disease?

Finally permit me to refer to the possible influence of focal infections in the production of angina pectoris. According to Clifford Allbutt's view angina pectoris is a manifestation of aortitis and tension of the ascending limb of the aortic arch which, causing stretching of the adventitia or fibrous coat, gives rise to the pain and sense of substernal oppression. The distinguished advocate of this very plausible theory mentions as the usual etiological diseases syphilis, rheumatism, and influenza. Since inflammatory rheumatism is a streptococcus disease, it is not unreasonable to assume that some focus harboring streptococci may be responsible for a chronic aortitis or atheroma which, lessening or impairing the elasticity and resistance of the aorta, permits stretching of its outer coat and consequent pain. Without wishing to postulate an authoritative statement or confirmation of a positive causal relationship between angina pectoris and focal infections, particularly such as harbor the *Streptococcus viridans*, I yet am impressed with this possibility, since in not a few instances of this formidable chest pain we have found septic foci in mouth, or throat, or both.

REFLEX DISTURBANCES DUE TO FAULTY DEVELOPMENT OF THE TEETH.

By J. MADISON TAYLOR, M.D.,

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Facts are accumulating of deep significance, in connection with recently directed attention to developmental faults in the teeth. These facts speak for themselves, and are becoming impressive by their mass and pertinence. The causes conditioning them while not so plain as they soon will become, lie in the genesis and evolution of growth forces, and these, so the evidence seems to show, depend in great measure upon altered functioning of the ductless glands and certain phenomena which are to be explained only by a yet fuller knowledge of the reflexes and reflex irritations involved.

For some years a branch of surgical art known as orthodontia has developed and absorbed the attention of a group of specialists who have already made great strides in solving the practical problems of malocclusion. The causes, especially fundamental causes, are but too little understood, as they admit, especially those of primary points of departure from the norm.

To the findings of these specialists we are deeply indebted and when to them shall be added those of experts in neurology, and in the rapidly developing science of hemadenology, we may safely predict large advances in the near future in methods of rescuing sufferers from mental and physical decrepitude, among whom are many hitherto fit for little more than isolation.

This assembling of observations, findings and scientific advances wherein the work of dentist and physician is directly correlated, is the function of the Henry S. Upson Foundation for

Research. So soon as we can secure cordial and universal coöperation and become as we desire, the repository of data, also contributors to aid and counsel, and above all, become the recognized organization and arena for candid discussion, we believe we shall make progressive and important contributions to human welfare.

The profession of medicine is beginning to appreciate more fully the significance of mechanistic factors, based in bio-physics; on the dynamics exerted by stress, strain, by undue and misapplied pressures long continued, and on deviations from normal adjustments, long applied to developing structures, notably where stress is long exerted upon nerve centers and excessive pressure on nerves in continuity, and the like. These irritations are peculiarly forceful in dental problems.

The nervous system has come to be recognized not only as the reflector of many currents of life but also as a regulator of those currents. The driving force of the nervous system seems to reside in forces manifested by or through the ductless glands. Certainly these internal secretions exert a controlling influence on processes of metabolism. The mechanism of their relationships is now being made clearer and its problems better defined and differentiated. It would seem a more candid understanding can be had through those clinical instances of combined mental retardation and nervous derangement and disease along with developmental faults in the teeth.

The problems arising deserve coöperative study from the standpoint of organic differentiation and the maintenance of such differentiation of growth and the limitations of growth and the management (conservation) of the sources of energy.

The underlying facts are in large part mechanical, however much they are biochemical. The key to clinical problems points toward the ductless or, as many call them, endocrinous glands or organs, whereby also control can be secured of differentiation and elaboration. When repressions or retrogressions occur, due to disturbances of these organs, they are incident to such rapid

differentiations that proper growth is interfered with, or deviated. Experimental evidence shows that thymus has the power to stimulate somatic growth, but lacks the power of inducing differentiations.

"The function of the thymus is to supply, through the agency of its lymphocytes, the excess of phosphorus in organic combination which the body, particularly the osseous, nervous, and genital systems, requires during its development and growth, *i. e.*, during infancy, childhood, and adolescence or later if need be."

". . . the specific symptoms or stigmata which indicate hypothymia. They are as follows:

"(1) Deficient development of the osseous system and deformities suggesting rickets or osteomalacia, due to deficient assimilation of calcium owing to the deficiency of thymic nucleins which take part in the building up of calcium phosphate. Deficient stature.

"(2) Deficient mental development, entailing various grades of idiocy due to insufficient production of thymic nucleins to supply the neurons of the central nervous system, during its development.

"(3) A low relative lymphocyte count owing to the inadequate formation of thymocytes."—Sajous, "Internal Secretions."

Thyroid on the other hand is capable of accelerating somatic differentiation, but not to stimulate nor to inhibit growth.

"(1) The thyroparathyroid secretion and the thyroid preparations used therapeutically act by increasing the sensitiveness of the phosphorus of all cells, particularly their nuclei, to the oxidizing action of the adrenoxidase, and thus enhance metabolism and nutrition.

"(2) They also, in virtue of this action, augment the autoprotective, or immunizing, power of the blood, by increasing the sensitiveness (as opsonin) of all bacteria, their toxins, endotoxins, toxic wastes, etc., that contain phosphorus to oxidation, and thereby to the digestive or destructive action of the complement, both in the blood and its phagocytes."—Sajous, "Internal Secretions."

The causes of malocclusion are also the causes of the effects, developmental and pathological, which accompany or follow. These effects are often mental retardation, physical stunting or morphologic anomalies and several forms of diseases not readily classified, among which epilepsy (spasmophilia) is one of the most graphic and pathetic.

Of the cases presenting at the dental dispensaries, Dr. Edward C. Kirk observes one uniform point advanced by the mother, viz.: an apprehension lest the child may be too nervous, too delicate, or not strong enough, to bear the strain of corrective procedures. This shows an instinctive awareness of a fundamental fact, that the condition is one of lowered vitality. There is usually also a history of adenoids, imperfect chest development and other stigmata of imperfect oxygenation and nutrition. (I quote here and later from an address by Dr. E. C. Kirk published in *Dental Cosmos* for June 1917.)

"The researches which have been made into the physiology of tooth movement by various investigators, particularly Noyes and others, and the accepted doctrine that the movement of a tooth shall be kept within so-called physiological limits—that is to say, that it shall be moved no faster than the resorption of tissue upon the one side of the moving tooth is compensated for by the construction of tissue on its opposite side—is another practical recognition of the necessity for the elimination of excessive mechanical stress upon the terminals of the dental branches of the fifth cranial nerve in the investing tissues of the tooth root.

"It is this latter factor—namely, the irritations of the terminals of the dental branches of the fifth cranial nerve in and about the tooth—to which I desire particularly to call your attention and if possible shed some light upon the importance of the principle which in certain of its aspects is already recognized by the orthodontic specialist, and which has led him to so conduct his operation of correcting the malocclusion in the class of cases to which I have referred that he shall not impose a

further source of irritation and an increased nervous stress upon the patient whose nervous organism is already bearing a burden of nervous stress upon the same nerve terminals caused by the malocclusion itself."

The malocclusions may be (*a*) accidental, traumatic, from caries, etc., or (*b*) constitutional.

". . . through hereditary influences affecting the general nutrition of the child or constitutional diseases in infancy which interfere with proper development and growth of the body, cause arrest of or interference with the development of the maxillary and mandibular bones with consequent malpositioning of the dentures. It is to these latter causes that by far the largest and most important group of cases of malocclusion owe their origin . . . ."

"The probable connection between certain types of malocclusion and certain associated stigmata of arrested development in other parts of the body, particularly of the osseous system, early attracted attention, more particularly that type of malocclusion known as the V-shaped arch and high-vaulted palate so often found in the case of the mouth breather."

". . . related as effect and cause, and defended in a paper by G. Bryson Delavan."

That rickets is the cause, A. B. Marfan is positive, and in a clinical lecture he advances the theory "that adenoid vegetations and enlarged faucial tonsils and the deformity of the maxillary arch have a common origin in a constitutional manifestation of rickets."

Marfan cites a powerful and interesting array of evidence to sustain his opinion. He rather dogmatically rejects the opinion of Hertoghe, of Antwerp, that the osseous deformities of rickets and the tonsillar and adenoid hypertrophies, regarded as stigmata of the same constitutional condition, are due to hypothyroidia.

"That the thyroid secretion is essential to the development of the fetus is shown by several facts. Myxedematous women seldom become pregnant, and when they do, in the absence of

thyroid medication they invariably abort. It is normal for women during pregnancy to develop an enlargement of the thyroid gland, which subsides rapidly after the child is born. By no means the least important function of the thyroid gland is that of fixing the calcium salts in the body. In order to permit of bone formation in the fetus, the mother is obliged to provide more secretion than under normal circumstances she requires, and the gland consequently hypertrophies. After the birth of the child, the increment being no longer necessary, the gland resumes its usual proportions. In some women this prolonged call of pregnancy has the effect of unduly exhausting the gland, and they are unable in consequence to suckle the child, for lactation is dependent upon a due supply of thyroid secretion. Such women generally become obese and lethargic, and remain so for varying periods until the thyroid has had time to recover itself. Judicious thyroid medication will frequently not only enable a mother to suckle her infant, but will materially shorten the period of her post-partum difficulties."

"Now, with regard to rickets I feel in a position positively to affirm that if all the symptoms of the disease are not due to thyroid insufficiency, then certainly its most salient features are. Especially does this apply to the bony phenomena which characterize its outward seeming. These phenomena are obviously due to inadequate osseous development, and as in the fetus so in the growing infant, thyroid secretion is essential to the full utilization of the calcium salts. It is known that the bony phenomena are due to a relative absence of calcium salts, and it is also known that these salts, given in large excess though they be, have no influence in arresting the disease. This is because the all-essential link is missing, the thyroid secretion, by whose means alone the ingested calcium can be so assimilated as to be incorporated in the osseous tissues."

". . . every case of rickets in which I have employed thyroid extract has shown such decided improvement as to leave no doubt in my mind that thyroid insufficiency is the main causative factor in the disease."

". . . a more extended experience confirms me in the views which I then rather tentatively expressed. These may be briefly summarized as follows: Adenoids and enlarged tonsils occur in children who have an adequate supply of thyroid secretion. The hypertrophic condition in each case is apparently the result of an endeavor on the part of the organism to supply an internal secretion as nearly allied as possible to the one which is lacking. If the hypertrophy is not very pronounced, and if it has not been very long in existence—great enough and protracted enough, that is, to produce complications, such as disease in the tonsils themselves or in the ears—then the exhibition of thyroid extract will cause their regression. It is only when medical means have failed that operative interference becomes justifiable."

Leonard Williams (*Folio Therapeutics*, Oct., 1909) sustains from extended observations the opinion of hypothyroidism and says:

"It would seem from the observations of the authors quoted that the deformation of the maxillary arch, or at least that particular deformation which we know as the V-shaped arch, the vôute palatine en ogive of the French, is primarily due to the arrest of development of the maxillary bones, which arrest of development is due to impairment of the normal process of bone calcification, and that this interference with osseous calcification is related to insufficiency of thyroid activity."

"The researches undertaken since the period (1907) when the conclusions which I have cited were published seem to indicate that the process of bone calcification is controlled more particularly by the hormone of the parathyroids rather than of the thyroid gland."

"Thus far there appears to be ample justification for the view that the primary cause of the V-shaped palatal deformity is a retardation of maxillary development induced by insufficiency of thyroid or parathyroid activity. We are, however, still confronted with the possible rôle which stenosis of the naso-pharyngeal space resulting from retarded development of the osseous

structures, the adenoid and tonsillar hypertrophy, may play in the increase of the deformity by compelling habitual mouth-breathing and thus introducing extrinsic mechanical forces as factors in the problem of palatal deformity.

"That the foregoing conditions may play such a rôle, and in no inconsiderable degree, appears to have been demonstrated by the researches of Baker, Bebb, and G. V. I. Brown, who have experimentally produced deformities of the palatal arch by permanently closing the nasal respiratory tract upon one side in young animals—rabbits, guinea pigs, etc. The importance of this mechanical factor of nasal stenosis and mouth-breathing cannot be by any means ignored, but in the light of the observations of the French authors whom I have quoted, the mechanical factor cannot be regarded as the sole cause of the deformity, but rather as a secondary or succedaneous factor superinduced by the original constitutional nutritional fault which originated the primary arrest of osseous development.

"If these views of the etiological factors responsible for the production of the V-shaped palatal defect be correct, then it would seem that those who, like Meyer, David, and Delavan, have advanced the nasal stenosis and mouth-breathing theory, and those who, like Marfan, Hertoghe, Williams, and others, have given prominence to the theory of arrested development from imperfect ossification as the result of hypo-thyroidism, have both seen the truth, but each only in part."

Finally I beg to call attention to the point made in the beginning by Dr. Kirk to the effect that among the sequelæ of dental deformity are their nervous relationships. These will in the near future be embodied in a report of the Henry S. Upson Commission, and will only be referred to briefly.

DISCUSSION OF SYMPOSIUM ON FOCAL SEPSIS.

Dr. POTTENGER: I do not feel at all competent to open this discussion, but there are a few things I want to say. I have been study-

ing visceral neurology and trying to correlate the pathological anatomy and the end results, and I simply want to say a few words in this connection. First, take the question of hyperthyroidism being due to these various focal infections. The physiological effect is very plain—stimulation of the sympathetics increasing thyroid secretion. We see this in tuberculosis, as a slight enlargement of the thyroid gland. Consequently the explanation is that the toxæmia causes stimulation of the sympathetics.

Another point is in relation to the tonsils. Some of you will recall a paper I read last year before the American Medical Association, giving a little different view from that which is generally taken—in fact my idea is simply to call attention to the fact that tonsillar tissue and adenoid tissue being lymphatic tissue has a value. It is not that I disapprove of the removal of diseased tonsils; but the principal defense of the little child is the lymphatic tissue, and this being true then that tissue must be preserved if possible until the child has developed a specific defense. On first coming into the world the child comes in contact with bacteria; these pass into the lymphatic tissue which produces lymphocytes that destroy the bacteria, hence these tissues are important in producing for the child a specific defense. So I would make an appeal to save the lymphatic tissue until the child develops a specific defense. Of course if there is a definite infection the tissue will have to be removed, but save it if possible until the child is four or five years old. Tuberculosis particularly takes on a tendency to chronicity about the sixth or seventh year, and up to that time there is very little specific defense in the child. The fact that the tonsils occasionally contain bacilli does not mean they should be destroyed. It simply means that the bacilli are passing through. It does not mean a focal infection of tuberculosis.

A point brought out by Dr. Babcock's paper is the relationship between a septic process in the adrenal glands and high blood pressure. The adrenal glands are always stimulated by anything that stimulates the sympathetic nervous system. Consequently when you have a toxæmia you will have increased adrenalin thrown into the blood. An interesting illustration is shown in the degenerations. We can have a temporary rise of pressure from adrenal secretion. Some have thought that the real purpose of the adrenals is to preserve tone, but they overlook the fact that adrenal tissue acts the same as the sympathetic nervous system. Now then, if we have something that is daily stimulating our adrenal tissue it is going to stimulate our sympathetic nervous system as well, causing continuous vaso-constriction, and this might produce degeneration. I cannot help thinking that this is one of the ways in which our hypertension, our thickening or degeneration of arteries may be brought about, just as I believe that the degeneration we find in a chronic septic process may be due to the

same thing. We know that a myloid degeneration takes place in chronic septic processes ..... So it would seem to me that the sympathetics may be the bridge between toxæmia and arterial degeneration.

Dr. MINOR: One of the difficulties of the clinicians seems to be in recognizing the earlier changes in the gums. I have often had dentists tell me that pyorrhœa existed where the ordinary clinician would not suspect its presence from the appearance of the gums. It is easy enough to recognize what the condition is from gums such as those pictured this morning, but those are late conditions, and I think it would be desirable if the dentists would give us a diagnostic ability to recognize them earlier. I had a case two years ago in which admirable results were obtained in the knee joints by taking out the tonsils, and yet a very competent throat man could not say that the tonsils looked diseased at all. There was no focus, and yet taking them out removed the symptoms in the joints. It is not always easy for a doctor to come to a conclusion as to the health of the tonsils, and as long as I do not believe that in the adult taking out the tonsils is a dangerous operation if it is done properly, I think it is wise to go ahead and take them out even though they look pretty good.

Dr. HANCE: Dr. Minor speaking of the removal of a tonsil and finding it normal afterwards brings into my mind the thoughts gathered from reading Dr. Thomas French's article on the examination of the tonsil by transillumination; and on looking at the pictures it struck me as a very practical method, because by this he was able to distinguish that there was a focus of pus in the tonsil. At present I am sending a patient about 40 years of age to him to know whether he advises removal of the tonsil before the patient goes to a sanatorium for a nervous condition. It has been a great problem for the ordinary physician to decide, when he looks at the tonsil, whether it is to be removed. For a couple of years I have hesitated to advise the removal of the tonsils of this patient, and I am waiting to find out how practical Dr. French's method is.

In regard to focal infection in the teeth I should like to say a few words about some cases I have had to deal with, in reference to the question of autogenous vaccine where there is an abscess formation. I have used this vaccine in a number of cases in which it has been futile—in most of them the reason was subsequently disclosed by the X-ray, showing that .there was a condition which the autogenous vaccine could not reach. I have one case in mind in particular—a young woman with magnificent teeth, who had lost two of them and was to lose a third. She was in the hands of one of the most prominent dentists in New York City, when her mother, not being satisfied, found a dentist who saved the third tooth, and in talking

with him he gave me some very plain advice in reference to what he was doing, which I shall speak of, as I think it illustrates the question a little. His explanation was that by means of the more advanced technique which has been developed the roots of the teeth can be cleaned of all pyogenical membrane, and it is possible to go into these cavities which have been disclosed by X-ray pictures and get everything cleaned out. As a surgical procedure he had perfectly cleaned out the cavity which then filled up with granulation tissue and in following out this method in the case referred to the tooth was saved. He showed X-ray pictures, showing the tooth which was the source of infection for the gum boil that had formed and which had also affected to a small extent the bony tissue around it. He said one thing, which may be known to all of you, that with the very fine instruments which he uses he is able to clean the roots of all the teeth down to the nerve where it enters the dental canal just as thoroughly as he cleans the external surfaces of the teeth. After talking with him I have formed certain opinions which have enabled me to give advice to the patients who come to me. The question of removal of teeth is I should say a very serious one in which we are to uphold the dentist, unless we know the dentist—for there are dentists and dentists. The man I have in mind would not consider removing the teeth until he had a thorough X-ray examination made and he himself had seen the pictures, which he submitted to my approval. The other dentist in whose hands the patient had been, who was one of the most prominent in New York, pooh-pooh'd the idea of X-rays and said the tooth must be removed.

I should like to speak of another thought that comes into my mind. The condition of the teeth when the dentist is through his work be sure that the molars are properly opposed to each other and that there is no rocking of the teeth in their sockets, which results from the points of one molar not fitting properly into the cusps of the opposed molar.

Dr. Perkins: I have had a case recently showing the effect of focal infection on development. A young man about 22 years of age came to me with tonsillitis. His health was very important because his father, who has a large business, looked to him as the only one to take it up. His father said, "I wish you could find out what is the matter with my boy. He has not been well since high school. I can't put flesh on him—he can't stand work." I told him it was probably tonsillar. The boy had enlarged tonsils and tonsillitis. I sent him to one of the best throat men in the state, who agreed with me. I told the father that if removal of the tonsils did not cure the boy I would have the teeth X-rayed; and as a matter of fact

they were X-rayed at once. One tooth showed evidences of trouble at the root. I sent the boy to one of the best dentists in Providence, who refused to remove the tooth and said that the condition shown by the X-ray was nothing more than you would get where there had been inflammation about the tooth. Then I took him to another dentist who removed the tooth and found a collection of pus. Within a year the boy had put on 18 pounds in weight and his health was excellent. The other case I still have under observation—a woman about 40 years of age. Her whole family history is exceedingly interesting. I treated her daughter, who had been out of school on account of heart disease and was exceedingly melancholic. I put her on thyroid extract, and her heart disease was cured and the melancholia disappeared after a few weeks. Then the mother came to me and said her husband wanted her to consult me because I had done so well for her daughter. I found that when 15 years of age she had been to a man who is one of our members, who said she had tuberculosis. She then saw another man, who said he could find no evidence of tuberculosis. For some time she spent her winters in the southwest. I found that quite a number of her teeth had never developed, and putting her on thyroid extract also has removed a good many of her symptoms. I have not yet had her teeth X-rayed but I want to have this done.

I am sorry I did not hear Dr. Anders's paper, but I should like to speak of one interesting case I have had—an autopsy on a woman about 45 or 50 years of age who had been sick about 6 weeks. The only symptom originally was pain in the neck running up into the head and pain in the left arm. She was examined by a neurologist, but he could find no explanation, and she then began to have night sweats. I was called in as the question of tuberculosis was raised. I examined her carefully but could find no signs of tuberculosis, and I was sure the symptoms were not from that cause. She died a few weeks later and I was invited to be present at the autopsy. This showed absolutely nothing except an adherent pericardium. I insisted that a further examination be made, and finally on going up under the tissues of the chest these felt a little thicker on one side than on the other, and a little creamy pus came out from a small necrosis on the lower border of the first rib. Absolutely nothing else could be found—perhaps a teaspoonful and a half of pus, which was apparently the cause of all her trouble. I could not explain it in any way except by a reflex action from an irritation of the intercostal nerve.

Dr. MARVEL: There was a point brought out in Dr. Taylor's paper which has been very forcibly called to my attention in my own practice in the last year. One case was that of a young lady who had a sudden swelling in the side of her face, accompanied by very

severe pain and general distress. A dentist was consulted and apparently no disturbance was found. After going through a series of X-ray examinations we found on the film that a tooth supposed to be the so-called wisdom tooth was in the position which Dr. Taylor illustrated and was pressing directly against the molar, which tooth was removed. The swelling all disappeared, and the patient was given decided comfort, without there being at any time throughout this disturbance any evidence of infection.

A little nearer home is the case of a medical student who in the past spring was very much disturbed by severe pain in what seemed to be the third molar, and which proved afterwards to be the third molar. He was for one week so distracted by the pain that he visited the dentist each day to have something done, but with no relief. The film in that case showed that one of the divisions of the root had turned under and was apparently pressing back against the opposite division, and when that was corrected the pain subsided and he has been very comfortable since.

The third and most interesting case is that of a dentist (?) about 35 years of age who had very decided pains in the arch of both feet and in both knee joints, who had gone the rounds consulting a number of physicians, but none could find any evidence whatsoever as to the cause of the disturbance. She came to me and after examining her very thoroughly and looking for the source of trouble I was about ready to give up the case as one in which I was unable to locate the source. I suggested that the teeth be X-rayed, which was done, and we found a condition similar to that spoken of in my first case. The tooth was removed, and whether as a result or coincidentally the distress all disappeared. These cases have a place in the general discussion of focal infections, because they are giving rise to a great deal of trouble without being associated with infection.

Dr. Anders brought out a point in his paper which I think should be emphasized, and that is that in many cases of disturbance due to focal infection there was some prolonged infection preceding, and that those conditions become only secondary or superimposed disturbances. I think we cannot lay too much stress upon this point, because we find in many of these cases, such as luetic disease, a chronic so-called rheumatism. There has been a preceding disturbance that has made the individual susceptible—the resistance has been so lowered that he very easily becomes the subject of acute inflammation.

Dr. Jacobi: I do not understand what was the cause of the death of the Providence case.

Dr. Perkins: Nothing was found except the small abscess from which we got pus, and then the nervous condition.

Dr. JACOBI: And she died from that? That would show that ven an autopsy now and then is not sufficient to give the cause of death.

That was a serious remark and I did not expect it would give rise to laughter. It happens in New York just as it will happen in Providence. We hunt and we do not find a cause of death, just as we do not find the cause of many cases of disease.

The two papers of Dr. Anders and Dr. Babcock have reminded me again, as others have done before, of the fact that the principal thing for the patient is to have a doctor who knows his business. Again I am serious. These two papers show that the men who wrote them are good doctors themselves. They know medicine. And I am saying this again for the hundredth time—that our doctors should know more than most of us do know in order to be able to judge of the real cause of a disease and make a diagnosis. Dr. Babcock's paper reminded me of that again. It was replete with knowledge, but I want to say here in a word or two what I consider a mistake in that paper. He spoke at some length of the tonsils with abscesses and so on. I believe it will belong to the duty of a good doctor in the future to know more about tonsils and of the neighborhood than we are in the habit of knowing now. That is a subject in which I have been responsible for several short papers. I repeat here simply that we still attribute too much importance to the tonsils as the cause of disease. I have not been convinced in all these years that the tonsils should be charged with responsibility for as much disease as they are. The tonsils as a rule are a body separated from the circulation. They are surrounded by very solid fibrous tissue which does not give rise to absorption. I want to repeat that from this solid tissue no absorption takes place. It is very hard for it to do so even when there is an abscess in one of the crypts. And that abscess need not be a cause of disease. It may be now and then, but in the majority of cases it is not. When you open it it may—generally will—heal up. There is no absorption as a rule from such an abscess—there is no absorption, and no danger even when you find tuberculosis germs in such a tonsil. It is not true that in most cases those germs are absorbed and go into the circulation. Germs like the tuberculosis germs when squeezed out from the crypt will go down into the throat and there be swallowed, or they will be spit out and will leave the oral cavity; so I still insist that crypts filled with tuberculosis germs are not dangerous. A large number of crypts surround the throat between the posterior nares and throat. Now the circulation is invaded by the glands surrounding the posterior part of the throat in the tonsil. The circulation is invaded not from the tonsillar tissue but from the surrounding glands. That has been overlooked. I have tried to impress it upon the profession but with not much success. I repeat

again that whenever there is a colony in the crypts they will be washed out, squeezed out or swallowed, and do no harm. Otherwise all of us would be in danger of sepsis all the time. That is what I want to say and why I have been induced to rise here again. I want to say that the best and most well informed doctor is not too good in every possible humdrum case we have to deal with. And I want to emphasize that the tonsils should not be charged with all the calamities of the human body.

Dr. PERKINS: I want to add one word in regard to the case I spoke of. The tonsils were not removed, and although the boy had had repeated attacks of tonsillitis for years before in two years since he has not had one attack.

Dr. ROBINSON: I think it is clear that in Philadelphia Dr. D. D. Smith should be credited with the pioneer work on Riggs' disease and it has been quite a usual thing for some years past for him to see in his office a number of advanced cases and one or two years afterwards see the same cases and observe the results. The thought I really wanted to present was that we surely have not fully arrived at our goal by recognizing a certain number of local causes of infection; that there is a trinity, one link of which has not been much discussed—that is, why some individuals with every evidence of advanced foci do not develop systemic infection as for example only a fraction of advanced tonsillar and advanced Riggs' disease cases.

I want to add a word in regard to unerupted tooth in a man of 68 who had developed glaucoma, who was under the care of Dr. Fox (?). The state of his teeth was gone into and an X-ray showed an unerupted tooth. By surgical removal of this tooth the glaucoma was reduced and his comfort has been much increased in the past two years. I think this is worth mentioning—the true cause of the glaucoma was the disease in the unerupted tooth.

Dr. MERCUR: I should like to lay stress on two points that have not been brought out. I saw a case a few days ago which illustrated the fact that in making a diagnosis we must know where the focus of infection is. In this case the man upon investigation had an infection and was sick, and the physician had discovered that he had albumen and casts in his urine and a very large liver, and looking for the cause of the trouble, found that several years ago the man had had bacillary dysentery, abscess of the liver and Bright's disease. The fact of the matter was that the infection was due entirely to the teeth. The man had an attack of acute myocarditis; he was dizzy and had fallen down the day before. He was a man of 68, perfectly healthy and well. He had an acute myocarditis from the effect of the focal infection in his teeth; the liver had backed up and had infected his kidneys, but the whole trouble was in his teeth.

The other point is a technical one in regard to an experience that occurred to me about a year ago. My experience has been that the average X-ray man does not know how to take a good X-ray picture of the teeth. I have in mind a very good dentist who suspected some trouble in the teeth of the patient and had X-ray photographs made, but they did not show anything at all. The patient, however, insisted on having the tooth drawn, and a large abscess was discovered which the X-ray picture did not show. I asked the dentist how it occurred that there could have been such a large abscess and the X-ray picture not show it, and he said that the X-ray was not taken right. Since that experience we have had a good dentist in every case where the diagnosis was in doubt take the picture himself. I think this is an important point to make—that the average X-ray man does not know how to take the plates in the right way so that they may be interpreted.

Dr. Daland: The whole subject of focal infection, touching as it does almost every department of medicine, has increased so much in importance that it seems to me it may turn out to be the important key that will unlock the riddle of diagnosis and etiology and open the doorway to a series of diseases in which hitherto we have only been able to treat after they occurred. It seems to me also that we should always keep in mind the fact that many focal infections remain local; and it is for us to determine whether or not a local infection has become systemic—a question not easy to decide, for the reason that a focal infection may be local in January, systemic in February and local again in March. And furthermore the diagnosis is rather difficult as regards systemic infection because, to my way of thinking, focal infection should be roughly divided into two kinds—that in which we think of the infecting organisms being transmitted to a distance in the form of septic emboli and that which we recognize as toxæmia without a septic emboli.

The question of toxæmia without septicæmia is important, and a continuous toxæmia without septicanum reduces resistance. Toxæmia can produce relaxation of the musculature, the cardiac muscle included. We also know how very remarkable is its effect upon the nervous system, more especially the sensory nerves.

Our concepts regarding focal infection should be broadened from another standpoint—that a focal infection may be latent, subacute and chronic; and we must also remember that multiple loci exists. It seems to me furthermore that we should keep well in mind the fact that local focal infection may become systemic recurrently.

The question of diagnosis of a focus of infection in, say, alveolar abscess, where or there exists a direct communication with a septic focus around a tooth, is easy. We likewise know that when drainage

has been continuous the systemic results are much less important than when the same focus is closed. Then we must keep in mind the fact that it is exceedingly difficult or impossible at times to discover a focus of infection. Not infrequently in the teeth there may be five foci undetected. In regard to the localization of the focus diagnostically it is well to remember that after the fortieth year the majority of foci of infection are located in the teeth or gums; whereas prior to that age the tonsils are the usual source of infection. In regard to interrelationship, not infrequently a pyorrhœa causes a secondary tonsillitis or a primary sinusitis causes a secondary tonsillitis.

There is another difficult point regarding diagnosis. I have seen tonsils hypertrophied, atrophied or normal in size in which I could not determine whether they were the source of systemic infection. In more than one case the removed tonsil has shown intra or sub tonsillar abscesses. The great frequency of septic infection following tonsillectomy is due to the fact that the tonsil is simply decapitated. There has been a sealing up of the crypt, bringing about a condition favorable to focal sepsis. When we receive a report from a dentist that the teeth are entirely normal and are not the seat of foci of infection or a similar report from a laryngologist regarding the tonsil or sinus, it is not final, because more than once the subsequent history of the case has demonstrated that the teeth or the tonsils have been infected or that an ethmoid or sphenoid sinusitis has existed unrecognized.

The beautiful pictures we saw last night illustrating pathological conditions in the teeth—and this seems to have been the chief subject of our symposium—bring up the question of what is the meaning of these disease areas round about the apices of the roots. There is no doubt but they represent varying conditions and are usually due to infections or emmanated to the loss of lime salts in the alveolus after infection has dissappeared. If we place a man absolutely rest at röntgenographic plates demonstrate that the bones contain less lime than is normally a clue. Furthermore, it seems to me that when we have in these infective teeth a streptoccus, the great question arises as to whether or not the individual will suffice systematic infection. The strange thing about this particular question is that while waiting, a little uncertain as to what to do, the individual may not suffer at all or a cholecystitis or viridium and degeneration, or a very serious crippling of the joints. In one case mild polyarthritis an infected root was treated for a number of weeks, and the patient was considered to be well as far as dentistry went. Nine months thereafter a severe cholecystitis occurred a multiple arthritis on a serious endoperi in myocarditis may occur. The removal of the tooth showed that the canal was filled with pubescent material, which of course of itself is not infective, but round about the root the Streptococcus viridans was

discovered. In about 10 days the cholecystitis disappeared. Eight months later facial paralysis occurred and then we overlooked focus of infection in a remnant of tonsils till after a tonsillotomy was discovered and removed. It is important to prevent infection of the vital organs —in other words, the loss of that one tooth, although regrettable, was far less serious than a recurring cholecystitis followed by removal or drainage of the gall bladder, with its consequences. I for one believe that today the treatment of peridental abscesses is not satisfactory. Drainage of this region through the mouth canal by the introduction of a little cotton seems to me to be entirely unsatisfactory when one considers the minuteness of the canal. There is a curious attitude on the part of the dental profession, and probably also on the part of medical men, in thinking that where there are no local symptoms no disease exists, and then waiting until some big thing happens, like inflammation of large and important joints, or an involvement of the heart, or kidneys.

Another point I should like to touch upon is in regard to the colon. In the past, prior to our more complete recognition of what focal and systemic infection means, the importance of the intestinal infection was raised to the first degree, but with the increase of our knowledge its importance from the standpoint of infection has been very materially reduced.

The next point I should like to dwell upon is the question brought up by Dr. Babcock. I may be entirely wrong but I am coming to believe that one of the commonest of all causes of arterial wall disease is infection, from one of a number of organisms from a septic focus, the commonest of all being a member of the streptococcus group. I should also suggest our minds dwell upon the question of multiplicity of etiology. Suppose we have an infection which is luetic and added a streptococcus infection, one can see how much greater would be the pathological result under these circumstances.

Dr. Minor: It seems to me that usually men talk about things they know the most about and neglect the questions they are less familiar with. The question of teeth tonsils and frontal and maxillary sinuses is not so very difficult a one, but I find we don't know so much about the infection of the gall bladder, prostate and abdominal organs in these conditions. I hope someone will give us more light upon this side of the question. It is very simple to examine and take out a tonsil, but to examine the gall bladder and remove it is a much larger question, and that is what I want to hear about. Furthermore I hope that someone can give me light, because I have none, on the question of other forms of intra-abdominal focal sepsis.

Dr. Anders: Mr. President, you have discussed the scientific aspects of the whole subject thoroughly. The subject of focal infection, let us bear in mind, is not a new one, but we did not fully

appreciate its significance until we had the benefits of the work of such men as Miller, Hartzell, Billings and others. There is no subject that is being more widely discussed, both in professional and lay circles at the present time than this—indeed I see danger of our becoming intemperate in the matter of ascribing all sorts of conditions without justification to focal infection. I believe that notwithstanding the great advance it marks in our knowledge of a large class of diseases and of widely different diseases, we are in danger of overemotionalizing its significance. Not long ago a young lady was brought to me who hadn't a single tooth in her mouth. She was a sufferer from epilepsy and her physician had advised that her teeth be erupted with the hope of curing her epilepsy. Can you imagine a more unfortunate combination than a double set of artificial teeth and epilepsy! Again, I know of a case, well authenticated, in which nothing was found wrong with the teeth on X-ray examination save that there was an embedded wisdom tooth, and yet that patient had been advised, and strongly advised, to have all her teeth removed. Fortunately when she came to the expert extractor, he refused to do it, and then began a scientific study of the case, which should have been done before. As I have said, we are apt to rush to the conclusion that we have before us an instance of focal infection and advise treatment before we have made a careful scientific study of the case. The position we should take is one of moderate conservatism with reference to this whole question. I am sure that we are greatly in need of higher standards of knowledge, especially with regard to the bacteriology of this condition. To my mind it is perfectly absurd to say in a case of systemic infection that because you have found a small abscess at the root of one tooth it must necessarily be the cause of the infection. It isn't enough to aspirate some of the contents of that abscess and find a streptococcus viridans present. We must, it seems to me, make an attempt to find what organism is present in the outlying infected structures in cases in which we know the particular organism that is present in the local focus. And finally, I have recently seen a number of instances in which there was an obvious cause other than septic foes present to explain the systemic condition, but which had been entirely overlooked because of this tendency to view disease from one standpoint only. We forget how often these local foci are perfectly innocuous. I spoke in my paper of the failure of improvement that often follows the eruption of teeth and removal of tonsils, and this is due to the fact that the true causes of the complaints in question, which are really obvious in many cases, have been entirely overlooked. I sincerely trust that we may as a profession maintain a conservative attitude toward this whole question until our knowledge shall have become more complete.

Dr. BROWNING: I agree with the preceding speaker that our knowledge of this whole subject is quite chaotic and that we are laboring under a disadvantage. I did not select this phase of the subject because I thought it was important. I can safely say that I have known more failures in attempting to cure disease by the removal of foci of infection than I have successes, and yet there are so many successes that we must follow the line indicated by the last speaker. I chose this field because it is one which has not been insisted upon. The mouth I showed because there was simply a red, very small margin, which we see so frequently, and because there was also a very intense infection shown in the lower incisors—and this earlier manifestation has usually been overlooked. It is a condition that is easily treated, and that was my object in bringing it before you. Of course my paper goes more into detail in regard to other phases. One phase has been brought out, and that is the danger which sometimes attends operations for focal infections.

Dr. TAYLOR: I am speaking solely from the standpoint of development. In this field I realize that we are dealing with a large number of conditions that may lie at the base of many of these difficulties. Let us look back far enough to the primary point and see whether we cannot pick up our trail. All the cases I referred to today are non-sensory, and in all the cases that come to us the non-sensory element is very important.

In regard to impacted teeth, this is obvious enough when the impaction is painful, but there are instances, especially in epilepsy, of which we see a good many cases, where there is no sensory element in the matter at all, and only by the X-ray are we able to find anything. A neurologist in one of these cases was exceedingly obstinate in admitting it could be infective. That one case I will mention. One tooth was impacted against another, and a third impacted against that. The middle tooth was removed, as affording natural mechanical relief. Our experts all said it was the other tooth that caused the trouble, and the attacks went on just the same. .This is simply a typical instance of the sort of thing that makes us realize that the expert must not only determine the X-ray findings but also what shall be done. And that brings up the nature and character of the dental treatment—not only extraction but various other forms of treatment—separation of the lower jaw brings relief in many instances. There are, as I have said, a number of points we need to know, but let us get down to one or two. One is in regard to development. There may be a constitutional inferiority—also individuals vary from time to time. The point I am studying with greatest particularity is the status of the development or anomalies of development whereby these patients are made more or less susceptible to conditions of this kind.

A PRELIMINARY REPORT UPON SOME AMBULATORY CASES OF TUBERCULOSIS TREATED BY TUBERCULIN.

By RICHARD COLE NEWTON.

As we learn more of the true nature of chronic diseases, the remarkable clinical similarity between tuberculosis and syphilis becomes more and more apparent. Certain interesting cases of concealed or latent syphilis have been reported lately. Stoll says (*Journal American Medical Association*, December 23, 1916) "that there is scarcely an untoward symptom which may afflict mankind that may not be caused by the *Treponema pallidum*."

Certain clinicians have recognized for years that syphilis is a disease of the family rather than of the individual, but it was not until the more general application of the Wassermann test that it became known how frequent and extensive prenatal luetic infections may be. Any apparently healthy child may harbor the organism; and a subsequent development of neurasthenia, hysteria, epilepsy, persistent and apparently unrelievable headaches, as well as the varying forms of malnutrition, etc., may be the unfortunate lot of the child of luetic parents.

Head (*American Journal Medical Sciences*, October, 1916) relates a case, of unfortunately too common a type, where a married woman of forty-five had become a confirmed neurasthenic, having been well and strong up to her twenty-fifth year. She had sought help for a score of years in this country and abroad; and, to quote Holy Writ, had "suffered many things of many physicians" "and was nothing bettered but rather grew worse." Finally the Wassermann test gave a positive result, and in a twelve month an almost miraculous change had taken place in this patient after appropriate antispecific treatment.

Head comments upon this case as follows: "Now here was a patient with lues, suffering from the infection but without a single recognized symptom or physical sign to be detected anywhere"; and so the gamut was rung on "neurasthenia, psychasthenia, and nervous exhaustion," although the patient in this case was "as surely syphilitic as though she had gummata in her liver or necrotic bone lesions."

He goes on to relate an almost precisely similar case in a young woman of twenty-three, except that the latter had never been robust. She had suffered, however, from nervous symptoms and malnutrition like the syphilitic patient just mentioned, and had a curvature of the spine beside. She also had suffered from physicians and surgeons. These latter had concluded a series of abdominal operations by a hysterectomy in 1912, for which no specific cause was assigned except that "it was considered best for the patient's general health." No pathological condition was found at the operation. This patient struggled on for years under a succession of illnesses, getting better of one attack of exhaustion merely to slip into another. She was always tired, sometimes coughed, but raised no sputum. Many consultants had examined her chest with negative results. In the past year she had lost fifteen pounds and had dieted herself to such an extent that she could scarcely eat any solid food. She had gastroptosis and a displaced right kidney. The Wassermann test was negative. Koch's tuberculin, however, gave a positive reaction. The great similarity of the symptoms to those of the luetic case just cited is most instructive.

Dr. Head has reported fifteen similar cases, which he terms "concealed tuberculosis." He is not ready yet to give the results of his treatment of these cases, which can hardly fail to be interesting.

I can give you a case of my own, treated by gradually increased tuberculin injections, which, except that I was obliging enough to leave undisturbed all the patient's abdominal and other organs, closely resembles Dr. Head's case in kind if not in degree.

Case I. Mrs. J. V., age 47, N. Y., married, housewife. I was called in May, 1916, to see patient's daughter, who was suffering from so-called nervous dyspepsia. On examining the chest, I found a slight difference in the expiratory note at the right apex. The subcutaneous tuberculin test was tried and found positive. The symptoms were in some respects similar to those from which the mother had suffered for years. The subcutaneous tuberculin test was then tried upon the mother also and proved to be positive. The latter's history is as follows: Both parents living and comparatively vigorous at an advanced age. No other cases of tuberculosis known to exist in the family except patient's sister, who is probably tubercular, and patient's daughter, as stated above.

On looking up the patient's cards in my office index I find as follows:

"August 6, 1902. Diagnosis 'Anæmia.' Complains of scanty, tarry, and delayed menstrual flow. Has pain in her limbs and headache, especially on awakening."

"January 29, 1903. Complains of pain under shoulder blade and across chest. Tongue coated; bowels confined."

"September 27, 1905. 'Nervous.' Dyspeptic. Has trembling fits. Pains in various places, etc."

"October 27, 1905. Nervous. Anæsthesia of buttocks. General nervous condition. Constipated."

"November 13, 1905. Neurasthenia. Pricking all over body."

"November 20, 1909. '*Malaria*.' Frequent nausea."

"May 11, 1911. Has improved considerably after taking orthoform, soda, and codeia before meals; and pepsin after meals; and stopping coffee. No movement of bowels in six days."

"February 9, 1916. Flatulent dyspepsia. Bad breath, anorexia, some cephalalgia, lassitude, etc. Sight of one eye nearly gone since five years of age; vision now somewhat improved. Divergent squint at times."

"July 15, 1916. Since tuberculin treatment was instituted, she can eat almost anything without pain or distress. She astonished herself and her family by eating ham and eggs with great relish and no untoward results, immediate or remote. She can sweep without the pain in her chest which she has had for years after that exercise. She does 'not seem to herself to be so old.' Is still pallid."

"October 30, 1916. Still some dyspepsia and some general malaise."

"November 6. Indigestion is better. Pain on left flank after hard work has returned."

"January 9, 1917. She can eat cheese and digest it comfortably; a test of gastronomic ability entirely beyond her for many years. Feels 'all right.'"

"January 16, 1917. Has carmine-colored lips, a mark of beauty which she has not shown for a number of years."

"January 30. Feverish from some unknown cause."

"February 6, 1917. Cough and cold still troublesome. No fever."

"February 13. Feels very well."

"February 20. No unpleasant symptoms. She is practically well and is constantly astounded at her own improvement."

Her daughter, who went to Chicago before quite completing her course of tuberculin under my care, was taken charge of by Dr. Kline of that city and the treatment continued. She writes home that she never felt so well in her life. This young lady was always tired—would fall down from weakness in her knees after exercise. Although lithe and graceful in her carriage and markedly quick in her movements, she was never able to take part in æsthetic dancing, like the other members of her class in college, because she was too weak. She was precocious mentally and studied very hard. She complains bitterly of the food provided in the college commons, saying that she had to depend on scanty supplies from outside or go hungry. And this at one of the leading women's colleges in America! The possible connection between the poor and insufficient food and the invasion of tuberculosis in this case is but too obvious.

I will not go into her history in detail inasmuch as it so closely resembles her mother's. At last accounts she weighs 106 pounds, a gain of 5 pounds.

I will recite another ambulant case which is interesting because of its length and its resistance to treatment.

Case II. Miss R. Aged 42. Single. Stenographer. Born in New York. Father died some years ago of aneurysm, hard

arteries, and nephritis. Mother and two brothers living and well. No tubercular history in the family. She had so-called "malaria" at sixteen years of age, which was "chronic" and exceedingly resistant to treatment—so much so, as my notes say, that it lasted for years. Finally the correct diagnosis having been made, a tour in a sanatorium was prescribed. This was followed by an "arrest," which, in turn, was followed by another sojourn in a sanatorium, and the same order of events repeated itself. After three such experiences of the "dietetic-hygienic" treatment, my lady says she will die before she will undergo another.

At one of the above mentioned institutions, she took tuberculin in a spasmodic sort of way, and invariably felt benefited from it. Yet as her digestion has always been of the well-known so-called "pre-tuberculosis" type; and as the food provided did not agree with her, she frequently had small febrile rises which she herself thought were due to indigestion and not to tuberculin. Yet the latter was promptly stopped at any sign of fever, and the patient was sent to bed whenever the oral temperature was 100 degrees or over. And there she had to remain and no tuberculin was given her until her thermometer steadily showed a temperature below a hundred degrees.

The late Dr. Janeway examined her lungs some years ago and told her mother that she had the scars of an old tubercular process in her lungs. Whether there was an active tubercular process there he could not tell without the use of tuberculin, which he was afraid to administer for fear that it might start up the disease afresh.

In 1902 she was treated for typhoid fever and was seen by several doctors. The case was finally reported to the Montclair Board of Health as tuberculosis. In 1910 she was again reported to the Montclair Board of Health as tubercular. About two years ago she placed herself under the care of a well known specialist in New York City, and finally underwent what is known as a "diagnostic week" at the Presbyterian Hospital. A popular consultant was in consultation; at the expiration of the week, she was told there was nothing discoverable the matter with her. She naturally concluded that the doctors thought she was "hipped" and there was nothing more to be done. With her increasing bad symptoms and the alternative of another sanatorium tour or a probable long and hopeless illness, she was much

discouraged. The history of this young woman for the past twenty-five or six years has been a succession of the advances and remissions of her tubercular infection. Finally her local medical adviser, Dr. Anna Smith, referred her to me for tuberculin treatment. On testing her subcutaneously with tuberculin, she reacted, and was immediately put upon the tuberculin treatment.

She had a severe local reaction and stated that her skin was so sensitive to iodine that she could not bear to have the site of the injection painted with it, as is usually done. After the reaction to the test dose of tuberculin, she had an attack of what we supposed was grippe; which was of moderate severity and precluded further tuberculin treatment for thirteen days. Since then she has never had a marked constitutional reaction; but treatment has been intermitted once or twice on account of a severe local reaction.

She has gained 13 pounds. Her headaches and backaches, which formerly never left her, have been forgotten. Her lips are red and her skin has lost its waxy pallor. Iodine no longer irritates the skin and cuts or abrasions heal rapidly—a thing that never happened in the old days. She has accomplished a great deal of work, doing for a large part of the year her own work and her assistant's; and she has felt well and happy. She declares that I cannot use too strong language in describing her case.

The following case of tubercular peritonitis seems worthy of recital:

Case III. W. A. Male, aged 15, born in New York. Residence, Bloomfield, N. J. School boy and assistant in a grocery store. Family history unimportant, except that his half brother died of tuberculosis at thirty. This brother had been "cured" of the disease with creosote when 17, and is alleged to have had syphilis in addition to tuberculosis. Both parents alive and well. I was called to this patient in August, 1916. He had fever, abdominal pain, vomiting, anorexia, and night sweats. He was in considerable distress. Yet as there was a history of overindulgence of a normally powerful appetite, he was given something for a summer diarrhœa and not seen again for three days. As his symptoms had persisted, I was recalled, and gave castor oil and quinine, and pronounced the disease probably "malaria."

The bowels, however, remained painful and there was more or less distention and flatulence. A history of a similar attack a month previous was given, which had subsided after home treatment with castor oil. In two or three days a boggy mass was discovered in the right lower quadrant of the abdomen. This was not sensitive to the touch. There was some pain on micturition, none on defecation. As it seemed about time to call in a surgeon, Doctor James T. Hanan, of Montclair, kindly saw the case in consultation. He advised that the boy be sent to the hospital, and gave it as his opinion that we were dealing with one of three abdominal conditions, viz., syphilis, tuberculosis, or cancer. The Wassermann test proved negative, while the subcutaneous tuberculin test was positive. Tuberculin treatment was instituted at once. In fact the case had begun to improve from the test injections, and the mass in the abdomen, which had reached to the umbilicus on August 28, 1916, by the fourth day after the first (test) dose of tuberculin, had so far subsided that it could not be palpated through the abdominal wall. On September 5th the patient was well enough to leave the hospital. His weight was $123\frac{3}{4}$ pounds, a loss of 20 pounds since his original attack six weeks before. A softening mass as large as a baseball could be felt, per rectum, in Douglas's Cul de Sac.

The tuberculin has been continued practically without interruption to the present time. The patient gained 5 pounds in weight from September 8 to September 11, and this increase (at a smaller ratio) has continued until now he weighs $146\frac{1}{2}$ pounds—$3\frac{1}{2}$ pounds more than when his sickness began.

By December 11, he found that he could run without pain or stiffness, and practically as well as he ever could. As he expresses it, from August to December, he had "forgotten how to run." Feels perfectly well. He has attended school regularly and worked in a grocery store afternoons and Saturdays since October 1.

On February 23, 1917, Dr. Hanan kindly examined the abdomen and found no trace of the former exudate.

One other case has recently come to my knowledge which is illustrative in its way and worth quoting. Although it is not an ambulant case, nor under my treatment, it has made such a strong impression upon my mind that I feel you will pardon me for alluding to it.

Case IV. A friend of mine, of about 63, consulted one of the leading genito-urinary surgeons in the country regarding an enlarged prostate. This was supposed to be carcinomatous and an effort was made to excise it. When cut into, the condition was found to be tubercular and was secondary to a tubercular peritonitis. The incision necessitated the formation of an artificial anus through the perineum.

Whether the patient recovers or not, his condition has perhaps been very greatly aggravated by an operation that might not have been undertaken at all if the true nature of the morbid condition had been ascertained, as might easily have been done before operating.

If the subcutaneous tuberculin test, as well as the Wassermann, were tried in every chronic case before operation, much information of the greatest possible value to patient and operator might be elicited. In the writer's opinion, the failure to make use of both these diagnostic agents is, in the light of our present knowledge, entirely without excuse in cases such as the above mentioned.

It is gratifying to note that the routine use of the Wassermann test is increasing in hospital and private practice, and there is good ground for the assertion that the employment of the subcutaneous tuberculin test, in chronic cases similar to those which receive the Wassermann test, will increase in frequency just as the use of the latter test has.

REPORT OF A CASE OF GASTRO-ILEOSTOMY IN WHICH THE ANASTOMOSIS WAS TAKEN DOWN THREE YEARS AFTER THE ORIGINAL OPERATION.

By DR. WM. H. MERCUR,

PITTSBURGH, PA.

Synopsis.

A description of a case of a woman, aged 38 years, who was operated upon for severe and repeated gastric hemorrhages. Operator stated he excised the ulcer and did a posterior gastro-enterostomy. Hemorrhage ceased after operation, but vomiting at stated intervals still continued. Vicious circle being suspected, her abdomen was fluoroscoped and the barium meal was seen leaving the stomach by two openings in the greater curvature of the stomach some distance apart.

From this and other data a diagnosis of a gastro-ileostomy was made, which was confirmed by a subsequent operation in which the gastro-ileostomy was taken down, an end to end ileostomy was done, and opening into stomach was sewed up. Patient recovered and is now at work.

Interesting clinical data were observed as the result of having so much of her bowel cut off from normal digestion for so many years.

Miss N. D., female cook, single, whose previous history was negative and whose family history was good, with the exception of one brother, who has had gastric hemorrhages for years, began to have gastric hemorrhages in 1909. These hemorrhages continued on and off for four years, patient having sometimes as high as three or four in one month. Several medical cures were tried in the meantime in various hospitals without any benefit.

Finally she was operated upon on July 11, 1913, in a small town in New Jersey. The hemorrhages ceased after operation, but three months later her former symptoms of pain, hyperacidity, constipation and vomiting returned. The latter symptom was the most persistent and it continued at intervals until I first saw her on June 11, 1916.

In addition to almost daily attacks of vomiting her principal symptoms at that time were burning pain in her stomach and obstinate constipation. Her weight at that time was 122 pounds, a loss of 20 pounds from her former weight of 142 pounds. During the period prior to her first operation, when she was losing so much blood, her weight got down to 90 pounds. Upon investigating her diet I found that it consisted practically of nothing but large quantities of strong tea with a good deal of sugar, a small amount of toast and bread with occasionally a little meat and rarely of very small quantities of green vegetables. She was pale and anemic looking and it did not seem possible that any one could exist upon the small quantity of protein, fats and carbohydrates which she was ingesting. A rough estimate made at that time showed that she was taking daily less than 40 gm. of protein, not more than 20 gm. of fat and that the total caloric value of her food did not exceed 1,500 calories. By encouraging her to eat a little more and by relieving her acidity with atropine she improved slightly and was able to keep at work. The most successful addition to her food at that time was Lenz Diet, a food in which the protein content is very high. The most careful regulation of her diet at that time, and its restriction to very moderate amounts of certain kinds of liquid and semi-solid food did not, however, prevent her from having more or less regular attacks of vomiting As the patient had not been given any information concerning the nature of her operation it was inferred that the recurrent attacks of vomiting were due either to a stricture of her pylorus or to a vicious circle resulting from the operation. A letter to her surgeon resulted in the following answer: "I operated upon Miss D. for the relief of gastric hemorrhages and found an ulcer near the pylorus and free blood in the stomach. With the aid of the house staff I excised the ulcer and did a posterior gastro-enterostomy."

The patient informed me that she was on the operating table from 8 A.M. to 12:30 P.M. so whatever was done surgically was

not done hurriedly. Patient continued at work and did fairly well until July 30, 1916, at which time her weight was 116 pounds.

Early in August she went to a hospital for a few weeks and with rest in bed gained 7 pounds. Upon returning to work again her former symptoms returned and when next seen by me on October 12, 1916, she had been vomiting almost constantly for three days. After a week's rest in bed was again able to go to work. Early in November she got much worse and a fluoroscopic examination was insisted upon. A very curious state of affairs was now found: the stomach emptied rapidly, but the barium meal was seen distinctly leaving by three openings, the pylorus and from two openings in the greater curvature of the stomach. This seemed most puzzling to interpretate, but a subsequent study of the X-ray plate and after learning from the patient that she could not stand up straight for several months after her operation without feeling a dragging sensation, convinced us that we had to deal with a gastro-ileostomy.

The question of what to advise the patient to do now became acute. Her means were such that she had to work if possible, but when she did the amount of food which she was able to take and assimilate without vomiting did not enable her to do so and she was gradually losing weight and becoming more anemic as the result of a badly balanced food supply. To advise a surgical operation as a cure did not seem, under the circumstances, to be very encouraging and on the other hand if she was allowed to go on without one she was almost certain to get worse and worse, so that later an operation would be counterindicated and useless.

Her consent to an exploratory operation was finally secured and it was performed at the Passavant Hospital by Dr. Wm. M. Campbell whose report of his operative findings follows:

"On opening the abdomen a mass of very firm adhesions was encountered, first of the omentum to the peritoneum, second adhesions of the omentum of the stomach, and transverse colon.

"After freeing as much of the adhesive mass as possible I found that the ileum had been anastomosed to the greater curvature of the stomach for a distance of about $6\frac{1}{2}$ inches, leaving the proximal as well as the distal ends of the ileum, which were about 5 inches in length, extending from each end of the anastomosis to the mesocolon.

"This portion of the ileum was so adherent that I was com-

pelled, for the safety of the patient, to cut each limb of the ileum above the mesocolon and do an end-to-end anastomosis at this point, and close the $6\frac{1}{2}$-inch opening in the greater curvature of the stomach. The pylorus was found to be normal and patulous.

"In this case I am of the opinion that the ileum was drawn through an opening in the transverse mesocolon and anastomosed to the stomach in the lesser peritoneal cavity, there being a loop of ileum between the stomach and transverse mesocolon with the two 5-inch limbs acting as the connecting links between the stomach wall and the transverse mesocolon or perhaps the stomach and mesocolon were not stitched together after the anastomosis, allowing the ileum to be drawn through the opening in the mesocolon to the extent stated."

The pylorus was found to be perfectly patulous and no signs of any former ulcer could be found. The contrast between the blue color of the ileum, which was plastered along the greater curvature of the white stomach, was most marked. As the double loop of the ileum which had been pulled through an opening in the transverse mesocolon could not be reduced owing to dense adhesion, it was a question what was the safest of two surgical procedures to follow. To first take down the gastro-ileostomy and do an end-to-end anastomosis of the ileum above the transverse mesocolon or, second, to leave matters as they were above the mesocolon and open the abdomen low down and attempt a lateral anastomosis between the two loops.

It was decided at the time that the first procedure would give her the best chance of recovery and this was done. The entire operation, which was most skillfully done, lasted two hours and 25 minutes. She suffered with a good deal of pain after the operation, but then did well for two days, when she developed a post-operative ileus, which yielded finally to repeated doses of 2 c.c. of pituitrin combined with strychnia sulph., gr. 1/30, and followed by repeated high enemata.

After this complication was relieved she convalesced satisfactorily and was able to be out of bed on the twelfth day and was soon able to leave the hospital and return to her home.

Her convalescence was further delayed by a deep stitch-hole abscess which developed nearly a month after the operation. She was able to return to work on January 7, 1917. From this

time on until April 13, 1917, she was able to attend to her duties as cook and to gradually add a little more to her diet; on the above date, however, while running in her kitchen to answer a call she had a very bad fall, after which she was not able to eat anything for several days without vomiting.

Since then she has been steadily at work, but she now has occasionally attacks of vomiting and occasionally severe headaches which seem to be associated with her constipation and are only relieved by Pluto Water, which seems to be the only effective laxative she can take.

A fairly careful search of the literature fails to show but two similar cases:

The first case was operated upon in the Mayo Clinic by Dr. E. S. Judd in 1912 and died. Correspondence with Mayo Clinic follows:

PITTSBURGH, PA., April 17, 1917.

Dear Dr. Mayo: I am just about to report an interesting case in which when taking down a gastro-enterostomy it was found at operation that the former operator had done a gastro-ileostomy instead of a gastro-jejunostomy. He has picked up the ileum close down to the ileocæcal valve and attached it along the greater curvature of the stomach for a distance of seven inches. As the pylorus was found patulous and in good shape an end-to-end anastomosis of the ileum was done, and the seven-inch opening into the stomach was closed up. Patient made an excellent operative, but a slow dietetic recovery, owing to the extent of the vicious circle, and that over three years has elapsed since the original operation.

While I was in Chicago yesterday I was discussing the case with Dr. Frank Smithies, and he told me that about four years ago when he was with you that he had seen a similar case. He did not remember the man's name, but he was from Texas, had lost 50 pounds in weight in six months, and was operated upon by Dr. Judd. Patient died. Prominent symptom was fecal vomiting.

Could you send me some further operative data about this case as I would like to mention it in my paper? And greatly oblige.

Yours sincerely,
WM. H. MERCUR.

DR. WM. MAYO,
Mayo Clinic,
Rochester, Minn.

MAYO CLINIC,
ROCHESTER, MINN.

April 26, 1917.

DR. W. H. MERCUR,
Pittsburgh, Pa.

Dear Dr. Mercur: Your letter to Dr. Mayo describing the gastro-ileostomy has been referred to me. I remember the case very well. The man came here in 1912, having been operated a year and a half previously at his home. This man whom Smithies mentioned came here in just about as bad shape as possible. Since the operation he had lost 50 pounds. He was practically unable to take nourishment in any form—he would vomit his food almost immediately, and had been gradually losing weight.

The operation was not especially difficult. On disconnecting the gastro-enterostomy I found that the opening had been made in the ileum within not more than 5 inches of the ileocecal valve. The openings were closed and he apparently seemed all right for a few days and then just gradually grew weaker and died, probably from acidosis. As I remember it, an autopsy was performed but everything was found to be in good condition, no obstruction, no leakage, and no cause could be found.

We have disconnected gastro-enterostomies made in almost all parts of the small bowel, but never before had we seen one quite so close to the ileocecal coil.

I trust that the information which I have given you will fit your needs.

Very truly yours,
(Signed) E. S. JUDD.

The second one was done in Baltimore in 1913 by Dr. Frank Martin on a patient of Dr. Albert H. Carroll, a colored woman. She made a good recovery. This case was published in detail in the *Annals of Surgery*, Vol. 61, 1915, page 557, and also in the *Maryland Medical Journal*, August, 1914. Those interested in this interesting subject are referred to his original article.

Dr. Carroll's case has many points in common with the case herewith reported. Both of the patients were cooks and both comparatively young and in both the anastomosis of the ileum was made very low down. They both remain obstinately constipated.

Dr. Carroll discusses in his article the question of how much food still continues to go through the pylorus after a gastro-enterostomy.

In the case reported it would seem hardly possible that she could have been nourished even as well as she was if she had received her food supply solely in the distal end of her ileum.

ASTHMA: CONSIDERED IN ITS RELATIONSHIP TO THE VEGETATIVE NERVOUS SYSTEM.

By FRANCIS M. POTTENGER, A.M., M.D., LL.D.,

MONROVIA, CALIFORNIA.

With our more extended knowledge of visceral neurology, we are able to form more definite ideas regarding so-called visceral neuroses; and such conditions as hay fever, asthma, hyperchlorhydria and spastic constipation can now be studied from a standpoint which offers to help solve many of the questions regarding them which have heretofore been misunderstood. These various clinical manifestations have been heretofore classed among the diseases of the organ or part affected; but our newer knowledge makes it plain that this is a mistake. They are evidences of reflex action. These phenomena are manifestations in different organs of the same syndrome—that of increased vagus stimulation. To make this clear it is only necessary to recall our physiology.*

The Vegetative or Involuntary Nervous System.

That part of the nervous system which is not under direct control of the will is called the vegetative or involuntary, sometimes also the autonomic. It supplies:

1. The subdermal musculature (pilomotor muscles and muscles of sweat glands).
2. Heart and blood vessels.
3. Muscles and glands of the respiratory tract.
4. Muscles and glands of the gastrointestinal tract, including the salivary glands, liver and pancreas.

* Pottenger, "The Vegetative Nervous System in Relation to General Medicine." Read at Annual Meeting of California State Medical Society at Coronado, April 17–19, 1917.

5. Genital tract.
6. Urinary tract.
7. The ciliary body, pupil and lachrymal gland.
8. All other smooth muscle and glandular structures of the body.

Throughout the entire body, the principle of action and counteraction is evident. This we see in the flexors and extensors; in inspiration and expiration; and in vasoconstriction and vasodilatation. Normal function is preserved by maintaining an equilibrium between the acting and inhibiting forces.

The vegetative nervous system is divided into two distinct divisions, one originating in the medulla, bulb and sacral portion of the cord, the other in the thoracic and upper lumbar segments of the cord. These two divisions are antagonistic to each other. They each give fibers to most of the important internal viscera and structures, including the heart, possibly some blood vessels (all receive sympathetic fibers), bronchi, stomach, intestines, liver, pancreas, genital and urinary organs. In each structure supplied by both, one produces action, the other inhibition. That division coming from the thoracic and upper lumbar segments of the cord is called the *sympathetic* nervous system; the other is called the *greater vagus* (Eppinger and Hess). The term *greater vagus* is applied to all the vegetative fibers that do not belong to the *sympathetic* system, and is named for the tenth cranial nerve, which is the most extensive nerve, in its distribution, of the division. It comprises the vegetative fibers in the third, seventh, ninth and tenth cranial, and those in the pelvic nerve. When these two divisions of the vegetative nervous system are in normal tone, normal physiological action takes place; but when one is overstimulated the equilibrium is destroyed and pathological action results.

Antagonistic Action of the *Sympathetic* and *Greater Vagu*-Divisions of the Vegetative System.

The antagonism of these two systems shows in the following manner in a few of the more important structures of the body.

EYE: *Sympathetic* dilates pupil and decreases lachrymation; *greater vagus* contracts ciliary body and pupil, and increases lachrymation.

SALIVARY GLANDS: *Sympathetic* decreases secretion; *greater vagus* increases secretion.

STOMACH: *Sympathetic* decreases motility and secretion, including hydrochloric acid; *greater vagus* increases motility and secretion, including hydrochloric acid.

INTESTINES: *Sympathetic* decreases secretion and motility; *greater vagus* increases secretion and motility.

LIVER AND PANCREAS: *Sympathetic* decreases secretion; *greater vagus* increases secretion.

BRONCHI: *Sympathetic* relaxes muscle tone and decreases secretion; *greater vagus* causes spasm of bronchial musculature and increases secretion.

HEART: *Sympathetic* increases rapidity of action; *greater vagus* slows.

BLOOD VESSELS: *Sympathetic* causes vasoconstriction; *greater vagus* in some parts causes vasodilatation.

BLOOD-FORMING ORGANS: While it is not possible to be definite in this matter, yet it has been noticed that conditions which cause increased *sympathetic* stimulation are accompanied by leucocytosis, with the increase largely in the number of neutrophiles; while those conditions which cause increased stimulation of the *greater vagus* are accompanied by increased lymphocytosis, eosinophilia and decreased coagulability of the blood.

ASTHMA A PART OF THE SYNDROME OF INCREASED *Greater Vagus* STIMULATION.

With these physiological considerations it is evident that the symptoms of asthma belong to a group which results when the *greater vagus* is overstimulated. The principal symptoms of asthma are dyspnea and cough. These are due to an increased secretion in the glands of the bronchial mucous membrane and

a spasm of the bronchial musculature. Emphysema is also an accompaniment in many cases. There is also commonly an increase in eosinophiles in the sputum. This picture is clearly that of overstimulation of the vagus.

The rapid heart is due to *sympathetic* stimulation, and shows how nature in her effort to preserve the organism provides that the essential body functions be carried on. In asthma we would expect the heart to be slower than normal if the same vagus stimulation extended to the cardiac vagus; but, if it did, life would be endangered by failure of oxygenation; so, as diminished oxygenation results in acid retention, the vasomotor center in the brain is stimulated and vasoconstriction with increased rapidity of heart beat takes place and a partial equilibrium is maintained. While oxygenation may not be fully carried on, in fact rarely is, yet a degree which is sufficient to meet the immediate necessities is usually established and maintained until the emergency is passed.

Medical literature is filled with speculation regarding the cause of asthma. Many have thought that the associated bronchitis must be the cause. This idea has been strengthened by the fact that the secretion lessens as the spasm lessens. This may be explained, however, by the fact that both spasm and increased secretion are results of vagus stimulation. We are not in a position to say, however, that the presence of the secretion on the mucous surface does not further irritate the vagus endings and prolong the attack. This is possible. It is also possible that vagus stimulation might call forth a different response in the muscle fibers and the secretory glands according to the strength or other characteristics of the stimulation. The one fact that is of prime importance in understanding the nature of asthma is that the essential conditions, bronchial spasm, increased bronchial secretion and cough (dyspnea and emphysema being results), likewise the increase of eosinophiles in the sputum, are conditions which are met when the equilibrium in the vegetative nervous system is disturbed in such a manner that vagus tonus predominates.

There are also, doubtless, changes in the glands of internal secretions which result from this disturbed equilibrium. What they are and whether they are within normal limits of variation we do not know, but must leave for further study.

That vagus stimulation is the factor which is accountable for the symptoms cannot be doubted. This is the reason why atropin and adrenalin are the remedies of greatest value for relieving the asthmatic attack. Atropin depresses the action of the vagus and if it could be given in doses sufficiently large would undoubtedly relieve asthma; but its drying effect on the secretions is so marked that doses which are required for the relaxation of the bronchial spasm cause too great discomfort.

Adrenalin opposes the action of the vagus by producing the same effect in the tissues as would result from central stimulation of the *sympathetics*. I have listened through the stethoscope to a severe attack of asthma accompanied by many squeaks and râles, and noted it disappear and the breathing become smooth and almost free from asthmatic signs within three minutes after the injection of 20 minims of a 1–1,000 solution of adrenin.

Another observation which tends to show definitely that asthma is due to vagus stimulation is the manner in which asthma is, at times, influenced by toxemia. Toxins cause an increased tonus in the sympathetic system.* Several patients who have come under my care have been relieved of asthmatic attacks by intercurrent infections—one by a tonsillitis; another by tuberculous toxemia; and a third by a pneumonia. These I have discussed more fully elsewhere.† Whether a case will be relieved by atropin, adrenalin, or by such intercurrent toxemias as just mentioned, depends upon whether or not the sum total of the *sympathetic* stimulation, or the depressant action as produced by atropin, is able to counteract and inhibit the increased vagus tonus.

* *Ibid.*, "The Syndrome of Toxemia. An Expression of General Nervous Discharge Through the Sympathetic System," *Journal of American Medical Association*, January 8, 1916.

† *Ibid.*, "Clinical Tuberculosis," C. V. Mosby Co., St. Louis, 1917.

Causes of Increased Tonus in the Pulmonary Vagus.

Vagotonic Disposition.—The first fact bearing on this point is the one which has been emphasized in vague terms for many years, without any definite conception of its meaning, viz.: That there is a certain inherited nervous basis back of asthma. Studies in visceral neurology show what this is. Eppinger and Hess* have particularly visualized this condition under the term "vagotonia." While equilibrium is maintained in the structures supplied by the vegetative system by the tonus in the *sympathetic* nerves equaling the tonus in the *greater vagus*, there are many individuals in whom the balance is slightly or markedly on the vagus side. If not already on that side, it may be thrown to it by any extra irritation of the nerve centers by which their threshold of response is lowered. This often follows overwork, disease, worry, discontent, and other depressive emotions, and the neurasthenic and psychasthenic states. While nerve equilibrium may be maintained under ordinary, favorable states, it is quickly destroyed by conditions which produce harmful stimulation of the nerve centers. A vagotonic may not show the complete syndrome of vagotonia. Only one or a few structures may show the influence of overstimulation. Thus one patient may have increased irritability of the lachrymal glands and nasal mucous membrane and show hay fever; another, of the bronchi and suffer from asthma; while still others may show the symptoms most marked in the gastrointestinal tract and suffer from hyperchlorhydria, spastic constipation and intestinal stasis.

We conceive that a patient who is distinctly vagotonic might have, not only a widespread predominance of vagus stimulation, but also increased tonus, the expression of which is limited to one or more branches of the *greater vagus*. When limited to the pulmonary branches, asthma results. It is not beyond conception that asthma might exist because of a specially low

* Eppinger and Hess, "Vagotonia." English Trans. Nervous and Mental Disease Pub. Co., 1915.

threshold of response in the cells which give origin to the pulmonary branches of the greater vagus in an individual who is decidedly vagotonic. Under these conditions a much less stimulus is adequate than under conditions of normal nerve tonus. In spite of this assumption, however, I am convinced that when the etiology is carefully worked out that we shall find that either peripheral irritation of the pulmonary vagus endings or reflex stimulation from irritants affecting filaments of the *greater vagus* in other internal viscera will be found to be the predominant factors which increase the already existing predominant vagus tonus.

It has long been supposed that the gastrointestinal canal is especially liable to be the source of reflexes which cause asthma. Bearing on this point, I would call attention to the fact that embryologically the lungs are formed from a diverticulum from the intestine. This fact makes the respiratory and digestive tracts closely associated in their innervation. In my study of pulmonary tuberculosis I have found this fact very evident. The inflammation in the lung apparently causes reflex stimulation in the gastric and intestinal branches of the vagus more regularly than in any other structures outside of the respiratory tract.

Let us now turn our attention more particularly to the study of those factors which might either directly or reflexly cause increased pulmonary vagus stimulation and precipitate the syndrome recognized as asthma.

Causes Acting Directly on the Pulmonary Vagus.—It seems to me that it is probable that the condition of the atmospheric air is a factor in the precipitation of asthmatic attacks. Observation of a number of asthmatics at the same time (in southern California) has shown me that while all will not suffer attacks at the same time, yet several will; and further that the attacks seem to be precipitated by changes in weather. The factors in the atmosphere which seem to be potent in producing the attacks are apparently more active during changes from normal humidity to increased

dryness or increased dampness. It would seem that the musculature and mucous membrane of the bronchi are not able to adjust themselves readily to the changes, and that irritation results. Fog at times is sufficient to produce the adequate stimulus for an attack. Those who have leaned toward the theory that asthma is due to bronchitis have explained the attacks following foggy and damp weather as being due to an increased bronchitis.

It may be that dust and other irritants in the atmosphere are also factors in some cases. The action of all of these factors may be either direct on the pulmonary vagus; or, what seems more reasonable, upon the nerve endings in the upper air passage and larger bronchi; the asthma being a result of the reflex. It is doubtful whether an attack would be precipitated, however, if the pulmonary vagus were not abnormally sensitive.

Asthma, now and then, seems to be due to direct irritation of the pulmonary vagus from conditions within the thorax. I have seen attacks follow rapidly-forming fibrosis in tuberculosis, either with or without marked shifting of the mediastinum. It seemed that the direct irritation of the vagus endings in the lung was most probably the determining factor; yet, we cannot exclude the possibility of protein irritation resulting from the change in the pulmonary tissue.

Stimulation of the Pulmonary Vagus from other Viscera.— Irritations which occur in the upper air passages have just been mentioned; but there are many sources of reflexes which may cause increased tonus in the pulmonary vagus.

Asthma sometimes accompanies hay fever. In this case, it would seem that what irritates the nerve endings of the upper air passages locally, stimulates the pulmonary vagus reflexly.

Deflected septum, pressure, polypi and sinus infections at times cause asthma. The chief factor in each of these conditions seems to be a reflex stimulation, although in case of polypi and sinus infections, so-called protein poisoning may be a factor, as will be evident as our discussion proceeds.

Next to the respiratory tract, for embryological reasons, as previously mentioned, irritation in the gastrointestinal tract should be the most apt to cause reflex stimulation in the pulmonary vagus, and this has long been believed. Cardiac asthma is also spoken of; and we now and then see cases reported where the reflex cause seemed to be in the genital or urinary tracts. The various neurons of the *greater vagus* are closely bound through the ascending and descending fibers in the cord so that stimulation may excite reflexes in far distant organs. We know vomiting may be reflexly caused by ovarian and uterine irritation; why not asthma as well?

Anaphylaxis.—There has been a growing tendency in recent times to appreciate more fully than formerly that the body tissues often find themselves confronted with foreign substances which have passed through the normal protective defenses. Thus foreign substances may pass through the mucous membranes of the upper air passages; and undigested food may pass the protective cells of the intestinal mucous membranes. In the destruction of these foreign substances, certain reactions occur. One is recognized as toxic, another as anaphylactic; the former expresses itself through the *sympathetic* portion of the vegetative nervous system, the latter through the *greater vagus.* These I have discussed more fully elsewhere.*

When the substance which enters the tissues of the body is a foreign protein, sensitization of the body cells takes place. When the same protein enters again, the cells are better able to destroy it, because they are endowed with the power of producing specific defense. The destruction of the protein is accomplished by chemical reaction. As a result, nerve endings are irritated. When nerve endings of the vegetative nervous system in internal viscera are irritated, the *sympathetics* cause segmental

* *Ibid.*, "The Syndrome of Toxemia. An Expression of General Nervous Discharge Through the Sympathetic System," *Journal of American Medical Association*, January 8, 1916. Pottenger, "The Relationship of the Syndrome of Anaphylaxis to the Vegetative Nervous System." Read before American Association of Immunologists, April, 1917.

reflexes through the sensory and motor spinal nerves, while the *greater vagus* filaments cause reflexes in other internal viscera. Protein in the process of being broken up circulates in the tissues and may irritate the nerve endings of one branch of the *greater vagus* more than another.

The biochemical reactions which are associated with the parenteral elaboration of foreign protein cause the syndromes of toxemia and anaphylaxis. These are closely related to reactions which result from the breaking down and elaboration of native protein during inflammatory processes. Here we have foreign protein also in all cases of infectious inflammation. In the destruction and elaboration of the protein of one's own tissues during inflammation, the syndrome of toxemia is often present. The reaction which seems to be more particularly due to the cleavage and further elaboration of the non-toxic protein molecules, expresses itself not as the syndrome of acute anaphylaxis, but in more or less widespread vagus reflexes in other organs. The syndrome which is recognized as anaphylaxis, either acute or chronic, and the symptoms indicative of vagus stimulation which results from visceral inflammation, seem to be related, but the latter as a rule are less acute than the former. It is possible that the only difference may be the greater part taken by the cells in defending the body against foreign protein.

It has been my observation that patients who show increased tonus in the pulmonary branches of the *greater vagus* (asthma) are prone to have increased vagus tonus in one or more other organs. Hay fever, hyperchlorhydria, spastic constipation and intestinal stasis are commonly found in patients who suffer from asthma.

Study of asthma in relationship to the vegetative nervous system shows it to be in all probability a condition with a varied etiology. In closing I wish to quote my conception of asthma from one of my recent discussions:* "Asthma is a disturbance in function. It is an increase in tonus of that portion of the *greater*

* *Ibid.*, "Clinical Tuberculosis," C. V. Mosby Co., St. Louis, 1917.

vagus nerve which supplies the bronchial musculature and mucous membrane. Its cause may be as diverse as the irritants which may be applied to the nerve centers which give origin to the pulmonary fibers, or to the peripheral nerves that are in reflex relationship with them. As all toxemias which are sufficiently severe may produce the toxic syndrome, so all irritations, either reflex or central, which produce sufficient stimulation of the nerve cells giving origin to the pulmonary filaments of the vagus may produce asthma."

DISCUSSION.

Dr. MANGES: I do not think there is much that anyone can add. The subject has been put to us in a way we ought all to remember. Anyone especially interested in following it up further can spend his time no better than by reading the monograph by Eppinger and Hess. It emphasizes one point which all of these neurasthenics show—that is, instability. Where you have a patient who is unstable, probably neurasthenic, there is an underlying cause, and it is your duty, before labelling him in a great group of neurasthenics, to do him the kindness and yourself the justice of trying to find out what it is that produces the instability. One can work out these cases, and by following up the anaphylactic end he will be nearer an appreciation of the neurasthenic state than ever before.

Dr. BROWNING: I think we are under great obligations to Dr. Pottenger for the fine amount of work he has done and the effort he has expended in correlating and bringing these things to our attention in the manner in which he has. Living next door to him I have had the opportunity of hearing him present this question in many different phases; and what he said in regard to asthma is also true in regard to other conditions, and doubtless can be explained to some extent in the same way as some of the more prominent complications which arise in tuberculosis. It is in just those atmospheric conditions that we have increased hæmorrhage, and at the same time that we have excessive hæmorrhage we have excessive secretion, excessive cough. Where you have a large number of very advanced cases it is at this particular period that there will be a group of deaths, so to speak. And so this presentation opens up a possible better understanding of a large group of cases and possibly in some instances, regarding death. And it will prove to our advantage to go further along the

lines, to which Dr. Pottenger has called our attention in his discussion of the determination of an attack of asthma.

Dr. COLEMAN: I am sorry the meeting is so far over that we cannot devote more time to the discussion of this paper. I want to express my personal appreciation of it to Dr. Pottenger. I think it is extremely suggestive.

I should like to say in regard to the use of adrenalin that as in the case of many other drugs, each patient is a law unto himself. I have had illustrated to me in the last few months what Dr. Manges brought out, that it is not necessary to give large doses of adrenalin. I have got as much benefit from a small dose as from a large one, and on the other hand I have seen patients who required as much as 30 mm.

Dr. DALAND: I cannot refrain from expressing to Dr. Pottenger personally the belief that here we have a connecting link between certain parts of the body, acting more or less together, and that a more intimate study of many of these organs will make many things clear to us. I would ask Dr. Pottenger as a favor to let me have a copy of his paper and of his special diagrams. This subject was first studied a good many years ago by Eppinger, and excited a great deal of interest in Vienna at that time, but I do not believe that we of the American profession have given the matter the attention it deserves.

Dr. POTTENGER (closing): I realize how difficult it is to bring a subject of this kind before you. In talking of the vegetative nervous system, I had to leave out a great many things of which I would have been glad to speak, even down to the changes in the blood. For example, in tonsilitis, changes occur in the blood. What is it that produces this change? There is a bridge somewhere between the inflammation in the tonsil and the blood itself, which we must supply. We have had our minds too much on pathological anatomy and the laboratory changes in the various tissues and secretions, but it is necessary for us as clinicians to study the link which connects the inflammation with these pathological changes.

In regard to adrenalin, I did not mean to imply that 20 minums is the usual dose which I employ. The patient I mentioned had very severe asthma. One morning in calling upon him after he had taken five minums of adrenalin, I noted that it had only given him partial relief. He told me that 20 minums would give him complete relief, so we injected it and in three minutes there was hardly a squeak left in his lungs. The spasms had almost wholly disappeared and he felt quite comfortable.

There is one question I would like to ask Dr. Manges in regard to

the use of adrenalin,—is it harmful to continue its use over a prolonged period of time? I have seen a number of asthmatics who are relieved only by adrenalin and it is necessary to use it over a prolonged period to give them relief. Is the remedy going to produce more harm than the asthma?

The Harvard laboratories have recently brought out a very important piece of work with reference to pneumonia. They have found that deaths from pneumonia are not due to the strain upon the heart by the disease within the lungs, but the impulses that are carried through the vagus nerve. They found that dogs with pneumonia with vagus cut, carried on their respiration and circulation in a fairly normal manner. Death seems to be due to the continued harmful impulses which are carried to the respiratory center through the vagus nerve.

The instability of symptoms is explained by the relationship of the inflammation to the visceral nerves. All people do not possess equally stable nervous systems. There are a great many people who have an increased tonus in the vagus system under normal conditions. If such a nervous system explodes it is going to explode on the side of increased vagus tonus. Most neurasthenics are vagotonic. They suffer from hyperchlorhydria, hyper motility, and much gas in the bowel. Often increased tone of the circular muscles prevents the downward movement of the intestinal contents and produces stasis. These same patients sweat readily, their skin blotches the moment they are anxious or excited. The picture which is so common at the time of the menopause is that of increased vagus tonus, probably precipitated by the withdrawal from the circulation of the normal internal secretion of the genital organs, which increased the tonus of the sympathetic nerves or inhibited action of the vagus..

We must approach nervous patients in a sympathetic way. We must recognize that the threshold of response of their nerve cells is low. They have pain when they would not have it if their equilibrium were normal. They suffer from discouragement and worry where there is little or no cause. One will never make progress in treating this class of patients by telling them that they have no pain and that there is no cause for worry, etc., but if one explains to them why they have pain when there is no inflammation present, and why they have worries when there is not sufficient reason for it, then one will secure cooperation.

BLOOD-LETTING.

By WM. DUFFIELD ROBINSON, M.D.,
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Unless it be the use of heat and cold, no therapeutic measure antedates blood-letting. It had certainly become firmly established long before Hippocrates, 460 B.C.

As to the persistency of its use, history shows it to have been constant throughout the ages. At times looked upon of as almost as vital importance in disease prevention and disease cure as is food to vital sustenance, in other ages it fell into disuse and was almost entirely abandoned, as being without merit. In the earlier part of the nineteenth century its popularity again increased, and it was held in high regard.

A study of the history of its use shows such lack of accuracy of medical knowledge as, in the slow and retarded development of the evolution of the science of medicine, was the history of the day.

Why has blood-letting lived? It has lived because so many men of the first rank in medicine have pronounced it meritorious and efficacious. Its merits must indeed be great or surely the lack of reason and judgment in its use which its history shows, must have killed it.

From the acme of foolish extreme in its practise, to its almost total abandonment, it has come to-day to be extolled by the leaders in medicine as of the greatest value when properly employed.

The objects of blood-letting are: to bring about a change in the quantity of blood in the vascular system, generally or locally, to change its relative constituents, and to change its activity in the performance of its physiological functions. The usual

object in blood-letting is the reduction of the pressure of the blood in the blood vessels.

The pressure within the vessels when reduced does not remain so, but is promptly reëstablished by the inflow of lymph from the tissues, and at the same time the caliber of the vessels is lessened by the action of the vasomotor nerves. When the vessels are changed by hardening, and consequent loss of elasticity, as in arterio-sclerosis, the return to high pressure is long retarded.

When continued high pressure indicates that reduction of the blood pressure was wise, it is well to remember that the old removed blood is going to be replaced as promptly as possible in the respective subjects by newly made blood of increased functional value. In a subject of fair functional powers not many hours are required for the attaining of full regeneration.

About one half of the blood of the human body must be lost to cause death, and the loss of one fourth is, as a rule, promptly recovered from. It is impossible to empty all the blood from the body, as with the removal of one half the heart ceases to beat and the balance of the blood, ceasing to flow, coagulates in the vessels. Cerebral starvation always causes convulsions before death in bleeding.

Loss of considerable quantities of blood reduces the number of red cells and so the power of CO_2 elimination. Accumulation of CO_2 depresses the vagus center and so retards and weakens the heart, which in turn causes cerebral anæmia. If the loss of blood be continued beyond one half death results.

According to the statement of Heinrich Stern it has not been shown by investigation whether or not it is possible to direct blood to the brain by position of the body after half the blood has been lost. If the lost blood is replaced by normal salt solution, thus reëstablishing volume, cerebral anæmia and death will be averted.

It is of interest to note that in all latitudes and longitudes, in all altitudes, in all climates and temperatures, in all modes

of living, the blood of the human body is the same in quantity, quality, and function.

It must be remembered that all the metabolic processes of the body are carried on by the blood. Also that back into the blood flow all the surplus and all the products of metabolic and catabolic activities. From this fact it is seen that every breach of normal metabolism must show in the blood and also that vitiated blood must mean vitiated metabolism.

In certain pathological states large amounts of such metabolic and catabolic substances are found in the blood, and this must be a subject for consideration in blood-letting.

No more certain and effective, no more easily controlled physical remedy exists than blood-letting. When the indications for it are positive, if properly performed as to the amount removed, etc., it cannot fail. Blood-letting is the most rational and direct treatment in disturbed osmotic equilibrium, suspended transudation of blood plasma through the vessel walls, capillary sluggishness or arrested movement of the lymph from the tissues to the veins.

It should be remembered that the red cells generated after blood-letting possess greater power of resistance than the red cells removed by venesection, and that with each subsequent venesection the power of resistance grows stronger. It is also true that more blood cells are formed after blood-letting than were removed by the operation.

After venesection the coagulating property of the blood is increased, at least in the vascular area affected by the loss of blood the first concentration occurs, speedily followed by dilution beyond the original. This dilution is due to the absorption of lymph from the tissue and with it is taken in, the substance which causes clotting.

In reducing blood pressure venesection produces a more lasting effect than do the nitrites, electricity, or hot air. By venesection, with associated infusion of normal salt solution the viscosity and the percentage of albumen in the blood are reduced.

By bloodless blood-letting is meant a mechanical treatment by means of which the blood is accumulated in certain parts of the body and detained there for such period as may seem desirable, in this manner depleting during that time the other parts of the body. It has been named phlebostasis and is brought about by placing such restrictions about certain parts as permit ingress through the arterial system and prevent egress through the venous system. We have examples in the placing elastic constrictions about the limbs at their junction with the torso to bloodlessly bleed from the latter, and in producing local partial vacuums by cupping.

The efficiency of this treatment is so patent that the infrequency of its use is an uncomplimentary reflection, showing the proneness of the mass of the members of our profession to seldom go beyond the confines of the therapeutic agents which are most used and which require of them but little mental effort in the selection or in application.

By the exclusion of the limbs from the torso the field of the labor for the heart is reduced and it is given an opportunity by partial rest to rally. The longer the blood is confined to the limbs the less will be the interchange of gases between the retained blood and the tissue it drains and also between the retained blood and the general circulation. By repetition of this method increased arterialization is secured. Disturbances of cardiac compensation, dyspnœa, and like obstruction to normal circulation are promptly relieved.

A treatment for circulatory adjustment by this method may last for days, giving increased chance for repair. Dyspnœa is at once relieved in most cases for hours. By this method of relieving dyspnœa, diagnosis as to the cause possibly resting with the breach of compensation is made evident.

While discussing this subdivision of my subject—bloodless blood-letting—dry cupping must be recognized at its full value. This is only appreciated by those who by the familiarity of long usage have grown to know its full worth. Perhaps no one can

speak with such commendation of dry cupping as he who has experienced personal relief from pleurisy or from that expression of myelitis, lumbago, by the use of dry cups.

The relief is immediate, and lasts for some time, often being followed by complete cessation of pain and restoration to health. This bloodless treatment is more efficacious than morphia, which deadens the pain but does not cure, and, of course, it has none of the ill effects of that narcotic drug.

The short time limit permitted for the reading of papers in this society makes impossible the giving the details of applicability of the various forms of bloodless treatment and blood-letting procedures. Blood-letting is perhaps accomplished more frequently and by more physicians by means of leeches than by any other method. This is particularly true in ophthalmology, in extravasation from trauma, and in all anal inflammations.

A leech weighs from thirty to fifty grains and extracts about three times its weight in blood. The after bleeding from leech bites is said by Weil to be caused by a substance which it secretes impeding coagulation. As a passing note of interest, it is said that a leech may be made to loosen its hold by sprinkling salt on its tail. Fresh blood will not coagulate if leeches are placed in it.

We will now consider the major expression of blood-letting, namely, phlebotomy or venesection. This operation usually deals with the entire blood circulation of the body. Details of the methods of doing venesection are sufficiently well known to permit of their being omitted. If vein incision by small bistoury is the method employed, the cut should be crucial so as to cut both the longitudinal and the circulatory fibers.

Puncturing the compressed vein with a trocar is a much neater and cleaner method. By this means the skin wound is reduced to a minimum and the course of the flow of blood is more easily directed to the intended receptacle.

A short word about blood-letting in special diseases will complete my paper. It will be remembered that the work of the

heart is lessened by the reduction of the volume of blood and that it is further lessened by the blood being made thinner. It will relieve back pressure, as it were, causing lymph stasis and so will accelerate metabolism. After depletion excess fluids in parts, as in the lungs for example, are first taken up. Contained poisonous substances are at once removed, often saving life, as their toxic action during the long delay required if the stimulation of the normal secretory organs is depended upon for their removal is thus prevented. Blood-letting stimulates the blood-making organs especially the bone marrow.

It is essential that the heart should be supported by the maintenance of a sufficient volume of blood when there has been a removal of large quantities either purposely or by accident. The injection of normal salt solution is, of course, the method of choice.

Under the caption of blood-letting as applied to respective diseases in which it has been used, the first here to be considered is pneumonia.

Pneumonia.

In the blood-letting treatment of this disease, perhaps more than in any other, there have arisen enthusiastic adherents and bitter opponents apparently unwilling to be convinced by any showing, history, or opinion.

Blood-letting does not influence the lung inflammation, but it does remove the venous engorgement in the lesser circulation and so prevents edema. The heart is relieved of the embarrassment of the overfilled blood vessels carrying blue blood and of pulmonary edema. In this connection dry cupping and bloodless blood-letting by the other enumerated methods must be kept in mind.

The physician who feels that an excess quantity of blood to the extent of fatiguing the heart is present, and that the existing composition should be changed is the advocate of blood-letting in pneumonia, at times, of course, associated with intravenous introduction of normal salt solution. As soon as dyspnœa or

cyanosis develops the time has arrived for blood-letting; the relief is immediate.

When the inspiration of oxygen and the expiration of carbon dioxide are mechanically interfered with by the compression of the capillaries, when the pulmonary vesicles are choked and there is present some edema, blood-letting, in the opinion of a majority of the leading men of medicine to-day, is the treatment to be chosen. Relieve the heart of overwork and often the life of the patient will thus be saved. Saline infusion will thin the blood and so reduce the force of the heart contraction which is necessary to maintain proper circulation. Of course, a portion of the volume of blood may first be removed from the already over-engorged venous side of the pulmonary circulation by wet cupping or far better by phlebotomy, venesection, or phlebostasis.

Bleeding *will* relieve an over-distended right heart; drugs and other therapeutic measures may do so. The action of a mustard plaster is in its effects simply a bloodless blood-letting of the part adjacent to that to which it is applied, plus a reflex nerve action from irritation.

The whole story of Bier's induced hyperemia is but the story of bloodless blood-letting. Osler, Tyson, Anders, and Daland are among those who advocate the use of bleeding in pneumonia.

Daland notes that the integrity of the heart muscle has been reduced by the toxins of the disease and that thrombus within the right heart occurs when the dilatation is at its maximum, so to prevent clotting venesection should be done early in right-side dilatation. The infusion of normal salt solution may wisely be synchronous with the taking of the blood and so the tendency to clot formation reduced. The blood should be taken rapidly.

Pleurisy.

Dry and wet cupping are as magical in giving relief to the pain and overcoming the inflammatory process as is the hypodermic of morphia in masking the pain.

Eclampsia.

Dyspnœa, lividity, and engorgement of the veins should be immediately relieved by blood-letting and normal salt solution.

Cardiac Asthma.

Cardiac asthma is almost magically relieved by venesection if done at the proper time.

Atherosclerosis.

Atherosclerosis and aneurysm are most favorably influenced by blood-letting. By this means both the blood itself and the condition of the vessels are improved, rupture of the vessels in the brain prevented and the distressing symptoms of abnormal cerebral circulation removed. After apoplexy it will not injure, but will often be of marked benefit.

Among the many diseases and mal-conditions in which blood-letting has proved beneficial may be enumerated uremia, chronic and acute, puerperal eclampsia, narcomia, chlorosis, epilepsy, migraine psychoses, many eye diseases, plethora, insolation, gas poisoning, acidosis, ulcerations, local inflammations, etc.

Due acknowledgment is made to the writings of Anders, Babcock, Brunton, Campbell, Daland, Jacobi, von Jaksch, Levin, Loomis, North, Osler, Riesman, Stern, Waterman, Weil, Zmke.

DISCUSSION.

Dr. Minor: I think all of us recognize the value of blood-letting in certain conditions. The little operation of opening the vein is a very pretty one, but it means dissecting down to the vein and passing ligatures under it—and that too often makes the doctor shy off from doing it—he has gotten a little rusty, and then too the patient looks upon it as an operation. Therefore we know it is not easy to do. Anything that makes an operation easy to perform is good; and I have recently used a 100 c.c. two way cock syringe for this operation. I sterilize the skin and enter the vein with the needle. In this way you know exactly how much blood you are taking out,

can remove as many c.c. as you wish and the patient does not consider it an operation. I would strongly recommend this method to every one who wants to carry out the measure easily and simply. If this method is used bleeding will be done more frequently—and yet I hope it will never become as popular as it was fifty years ago.

In hypertension I am satisfied that occasional bleeding is one of the best methods at our command; and there are many conditions, that I need not dwell upon, in which it is of great therapeutic value. All it needs is simplification. This was drawn to my attention recently in the case of a woman with apoplexy. She was so stout that it was very hard to find the blood-vessel at all, and it was neces sary to cut through a large area of fat, which made quite a complex procedure of a small operation. Many men are held off from the measure for this very reason.

Dr. MANGES: I want to bear testimony to the value of bloodless phlebotomy in conditions where the rapid reduction of blood pressure is desired—for instance in hæmorrhage from the lung, or from the bowel in typhoid, I know of no more effective therapy than this simple method. Putting the Esmarch bandage on both lower extremities you can see at once what an enormous plug is removed from the circulation by the simplest possible method. I know surgeons who are most enthusiastic advocates of this measure; and I know of nothing better if one wants to get an actual, absolute reduction of blood pressure in a simple and direct way. But my feeling is just the reverse when it is considered from the other standpoint—that of the greater circulation. In high blood pressure patients I have repeatedly had blood-lettings done with the manometer on the arm, and have failed to see the slightest influence on blood pressure, even with large bleedings. You might say that the physiological effect of the blood-letting on the patient more than is shown in the blood pressure readings but when it has been tried repeatedly and the blood pressure taken immediately, and you see it uninfluenced at all by a large bleeding of 12 ounces or more you begin to lose faith in all that blood pressure was going to accomplish. I am no longer enthusiastic about blood-letting in trying to influence high blood pressure and allied conditions. There is only one condition where the greater circulation is involved in which I have seen remarkable results by this method, that is in polycythemia. Here we are dealing with other conditions, with an enormous increase in viscosity, and in the hæmoglobin; the whole volume is increased; the water is increased; so that when you do remove 12 ounces or more in such an individual you are influencing not only the blood volume but also chemical and other conditions.

To come to another point, when I hear of a recommendation of

infusion after a phlebotomy I fail to see what the phlebotomy is accomplishing. Phlebotomy is to reduce blood volume, and if you do an infusion as well, your total volume remains the same. You may say that you are changing the blood composition, but after all the total blood volume you are removing is very small. When it comes to the question of the lesser circulation there we are dealing with a different proposition, and we must not confuse the benefits from this measure with those we are supposed to get from its influence on the greater circulation.

I would close my remarks by referring to one condition in which blood-letting is held up as *the* great remedy—that is in apoplexy. In a very exhaustive discussion of the use of blood-letting in apoplexy, in Von Monakow's treatise he speaks on the subject of venous blood-letting in apoplexy, and ends by saying: "Let me tell you that the apoplectic individual is better off without blood-letting than with it. . . . The blood pressure changes going on within the skull ought not to be tampered with by blood-letting. The increase of tension may be one of the means nature has tried to reduce the bleeding." I simply mention this and refer those who are interested to that elaborate discussion. Enthusiastic as I was some years ago, my enthusiasm has waned and I have gone back to the simple phlebotomy, and to the use of blood-letting in conditions affecting the lesser circulation.

Dr. ELLIOTT: Where the syringe is not available the use of a large broad needle answers perfectly well; in this way in a short time you can get a large amount of blood.

Dr. MINOR: Is there not danger of cutting (?) into the vein by such a method?

Dr. ELLIOTT: No, I do not think so. This procedure is very simple, and the needle can be carried in the pocket and used if the syringe is not available.

Dr. ROBINSON (closing): In regard to the use of a syringe in certain veins, with the diphtheria antitoxin syringe I have never seen any ill effects.

As to the question of relieving heart fag (?) by changing the viscosity of the blood I think there must be less work for the heart.

In regard to the theoretical value of blood-letting when apoplexy threatens, we know the condition before apoplexy obtains; we have seen those symptoms followed by apoplexy, and after we have reduced the volume of blood we have seen these patients given comfort.

They will come to you, say once every three or six months, and insist on having a blood-letting; and I take that as a better test than the theory of what the pressure does in nature's effort to repair within the brain. I usually remove 32 to 40 ounces and never have any trouble at all. This is not much in relation to the whole volume and does no harm.

THE RÔLE OF THE X-RAY IN THE PHYSICAL EXAMINATION OF THE LUNGS.

By CHAS. L. MINOR,

ASHEVILLE, N. C.

The subject chosen by our President for this symposium today is, as far as my part of it is concerned, one on which I have already written so often that I feel some delicacy in handling it again, but there seems still to be so much uncertainty among medical men as to the exact value of the X-ray in the diagnosis of pulmonary tuberculosis, that the subject is certainly a timely one.

The addition to our diagnostic procedures of any new method is a matter of no little importance to the profession when we consider how great are the difficulties we meet in studying and diagnosing any disease, and how hard it is to draw correct conclusions from our observations. No discovery in the last century gave medicine such an impetus as Laennec's great invention of the stethoscope It was unquestionably the starting point in the development of our knowledge of the respiratory and circulatory diseases, and has extended the range of our powers of observation further than any other apparatus, not even excepting the thermometer and the microscope.

Out of it arose modern physical diagnosis, one of the chief glories of medicine today, and which is fundamental to the proper study of any and every sort of case. But despite all that the careful diagnostician can accomplish by the systematic use of our regular measures, every physician, especially if he is able to control his diagnoses at the autopsy table, constantly feels the limitation of these methods and dreams of other means to enable his fingers, eyes and ears to reach further and wring from

unwilling nature a larger number of her secrets. How often, when we have arrived at what we supposed to be a correct and acute diagnosis, has an autopsy made us feel not only the limitations of our powers of observation and of reasoning, but dissatisfied us with the deficiencies of our standard methods of examination. The laboratory is placing at our disposal every year many new and invaluable methods for studying disease, but the additions to our means of physical examination have been few and not often of great value. Indeed, were the results of physical diagnosis more accurate and positive, how many more ambitious young medical students could we draw to the beautiful and profound work of internal medicine who now are attracted away from it by the more brilliant but less intellectual work of surgery.

With the constant demand for further light on our diagnostic problems, it was most natural that, when Rœntgen discovered the X-ray, its diagnostic possibilities were at once recognized by the profession and applied especially to the study of intra-thoracic conditions. That it has been a wonderful addition to our diagnostic measures, we all know. It is no longer a question as to whether it is valuable or not, and those of us who have used it systematically for any length of time find that we could ill dispense with the aid it brings us in the solution of our problems.

But while its value is generally recognized, there still remains much uncertainty as to its position relative to the other and older steps of a physical examination, and it is not surprising, therefore, that our President has chosen it for the subject of discussion today.

In studying this question it is evident that one chief difficulty lies in the fact that owing to the elaborate and costly instrumentarium and the demands it places upon pocket, time, office space and special knowledge, the clinician and diagnostician, unless he works in a large hospital, has not usually been at the same time the Rœntgenologist, this work having usually been done by a specialist in the X-ray, who is as ignorant of the re-

finements of physical diagnosis as he is skillful in the use of the X-ray.

While, unfortunately, owing to the elaborate instrumentarium to which I have referred, it seems unlikely that a combination of the clinician and the X-ray man will be often found, I still feel that such a combination is most desirable if we are to draw final and satisfactory conclusions from the X-ray findings in any given case.

The clinician is too sure of the reliability of the regular methods, the X-ray man much too positive in his interpretation of his shadow pictures, and too many of his reports speak with a positiveness that could only be justified by a complete autopsy and draw conclusions as to pathological conditions which are by no means always verified by the subsequent course of events.

A further difficulty lies in the fact that most American physicians are afraid of the effect upon them of the X-ray should they use the fluoroscope, and that, therefore, it is here but little and irregularly used, whereas in Germany its use is general and, therefore, it is much more highly valued. We cannot properly estimate the value of something we are not thoroughly familiar with. Were it used more generally and in all cases, the profession would find how easily and quickly it is applicable in office work, and by using it in all cases would soon gather a skill in its interpretation which would make it of real value, whereas where it is only used at rare intervals it will be improperly interpreted and its value underestimated. Its use is not difficult or unduly costly like that of the stereoscopic plate, and in the *majority* of cases will give us as much information as the latter. If a man has the time to run a photographic laboratory in conjunction with his office, he will certainly find a stereoscopic outfit of very great value, and it is undeniable that in certain difficult cases where, as in miliary tuberculosis, the shadows cast are very small, or where the lesions are deep-seated, the stereoscopic plate can alone give us satisfactory information and a correct idea of the conditions in the lungs. But if this method is to be

used exclusively it is certain that only a very small number of the profession will have it at their disposition in such a way as to thoroughly master it, and, therefore, the larger part of the value of the X-ray will be lost to the profession. If, on the contrary, they use the fluoroscope regularly, which is perfectly possible to the diagnostician or general clinician, they will soon become thoroughly familiar with it and derive from it infinite value. If, however, as is likely in this country, the stereoscopic plate conquers the whole field, I would advise doctors in receiving reports from the X-ray man not to accept with too unquestioning a faith the positive statements as to the exact pathological conditions existing in doubtful cases, and to remember that after all, as has often been said before, what it gives us is simply a picture of the varying densities of lung tissue and, with the stereoscopic plate, their relations to the chest wall and surrounding parts, and that it is unwise for a physician to draw too positive conclusions as to the activity or inactivity, or the pathological nature of the process casting the shadows.

In these days we frequently hear questions asked as to the comparative value of auscultation, percussion, etc., on the one hand, and of the X-ray plate on the other, and some enthusiastic specialists seem to think that these older measures will soon have to take a back seat or be given up altogether, a very good proof that such men have no thorough knowledge of the work of Laennec, Piorry, Walsh, Gerhardt, Skoda, Flint, Leaming and many others.

That the profession should throw away tried procedures which have been shown to give us invaluable knowledge of the conditions inside the thorax, because we have learned how to transluminate the chest is an idea which would appeal to no well-balanced mind. What we must try to do is to use each new measure to add to and complete the information won by other measures, and, by a synthesis of all the facts gotten through each procedure, form a more perfect idea of the conditions in the chest, and I cannot believe that the time will ever come, or a

discovery so wonderful ever be made, as to do away with the necessity of using every assistance we can get in the difficult matter of intra-thoracic diagnosis. I have used the stethoscope for twenty-nine years, the fluoroscope for nineteen in every case coming under my observation and have learned to value both very highly, and I would not wish to have to make an exclusive choice; yet, if a choice had to be made, I believe that in the great majority of cases the stethoscope and its usual adjuncts will give us a more satisfactory knowledge of the activity and nature of the process and of its progress than the best X-ray, while the latter will give us an idea of the geography and extent of the trouble in the lung and of deep-seated foci which the former cannot begin to rival. In the classification of cases, in the discovery of extremely incipient chronic tuberculosis, in the recognition of beginning softening, or of extensions, the older measures are unquestionably of much greater value, whereas in the discovery of deep-seated lesions, of miliary tuberculosis, of interlobar pleurisies, of small collections of fluid, of cavity, abscess and bronchiectasis the X-ray is infinitely superior to these. The disadvantage of the X-ray is that in acutely sick cases, even in hospitals, and always in the home, the X-ray is not available. Light apical lesions, cases for instance with a little feeble or granular breathing and a very few fine, dry crackles on cough, are much better discovered and evaluated by our older measures, the faint clouding of the apex shown by the X-ray giving us information on the subject which cannot compare with that which we get with the ear, even though it is very valuable sometimes in revealing to us small foci of trouble situated deep in the apex, but I am not at all afraid to assert that light haziness of an apex, limited expansion, limitation of the field, if not accompanied by signs discoverable by delicate physical diagnosis, are of little diagnostic value as *showing any active trouble*, whereas such signs as I referred to above, even if the X-ray picture is negative, as it often is, are of positive worth.

As we all know, the large majority of our incipient cases have their trouble above the second rib, a lesion which from anatomical reasons we are able to explore by auscultation, percussion and inspection very fully and satisfactorily, and here it is seldom that the ear will not get ahead of the X-ray plate. Lower down, on the contrary, in the middle or lower part of the lung, we all know that we can entirely miss considerable foci of active trouble, quite large cavities, fair collections of fluid, abscesses, etc., by the older methods, and no physical diagnostician but knows how often he has been caught napping by fluid in the chest, which with all his care he has overlooked. Even here, however, the interpretation of the pathological nature of the trouble is much better made by auscultation and percussion than by the picture.

The realm then of the X-ray plate, to my mind, is in the discovery of abscess, of cavity, of fluid, of interlobar pleurisy, of bronchiectases, of tracheo-bronchial adenopathy, of miliary tuberculosis and of the distortions produced by fibrosis, but the incipient case in the upper lobe, where information is so essential if our treatment is to be of use, can be studied to far greater advantage by regular measures. I have hoped that those puzzling cases where a suspicious fever cannot be explained by any physical findings may be solved by the X-ray, but I have been disappointed here, not simply with the fluoroscope but by the plate, and our reliance has to be upon a careful clinical study of the case for two weeks to a month or often much more. The work of Dunham and Miller studying the spread of tubercles in the lung by combining autopsy and study of the stereoscopic X-ray plate is likely to give us much valuable information, but as yet it has not demonstrated that in a really incipient case it is superior or even equal to the human ear and finger. Dry pleurisy is not well studied with the plate, and while some have claimed that simple congestion of the lung can be demonstrated by the X-ray, it would take much and very perfect work to convince the trained clinician that such was possible. However, Garvin, of Raybrook, has recently shown by a most interesting series of

plates, at the Cincinnati meeting of the National Association, that the eruptions of what seemed justifiably to be called tubercles in small localized areas of the lungs accompanied only by temporary fever could be shown on the plate as small areas of fine globular shadows, which tended to disappear with improvement of the symptoms. But if our present views as to the pathology of pulmonary congestion are correct, such congestions, as they are called, are not accompanied by the eruption of many tubercles, but only show an engorgement of the fine blood vessels of the part, and certainly will not be registered upon the plate. The X-ray can give us early and invaluable warning of the development of trouble in the apex by the appearance of light delicate lines running from the sternum to the second or third rib outward and upward. I do not refer here to the much denser shadows which come later and which are almost always accompanied by distinct signs, but the fine delicate radiations where at most you will find slight breath changes, in themselves not enough to justify strong suspicion. But when such slight signs are accompanied by linear changes, we may be reasonably sure that before long we will find trouble in this spot.

To any but the unbalanced enthusiast, then, the X-ray will be regarded as a priceless addition to our means of examining the chest and not as a procedure which will supplant and render useless other and well tried measures. Like every careful diagnostician he will try to collect every fact he can regarding the case which he is studying and will not make a diagnosis until he has carefully correlated all these facts and combined them with a proper history and with the result of a careful study of the clinical course of the case, and from these will draw conclusions which in the majority of cases will be justified by time.

I have recently found in a book, just published in Paris, by Dr. Barjon of Lyons, entitled "Radio Diagnostic des Affections Pleuro-Pulmonaire," so excellent a statement of the real value of the X-ray that I may be pardoned if I quote it here somewhat in extenso, for I have seen nothing in German or in English

as good, and it says much better than I could, what I feel on the subject.

"However this apparent superiority (of the X-ray over the regular methods) should not inflate the X-ray man's pride or intoxicate him to the point of thinking he can walk alone and dispense with the older procedures. This would be a grave error. If the X-ray is more brilliant than the older methods, it has not been tested by experience as they have and must constantly appeal to their support.

"Indeed, to see is not everything. With good eyes and good apparatus it is easy to see, but it is a question of interpreting what one sees and drawing from this conclusions which can be used in diagnosis. This is not possible to everyone. This delicate interpretation demands a mass of very precise knowledge of anatomy, physiology and pathology. This is why the radiologist must be a doctor. I would say more, to be a good radiologist it is not enough to have the title of doctor of medicine; one must have a thorough knowledge of sick people; one must know the evolution of disease, the complications that may aggravate them; one must be, in short, an excellent clinician. It is only thus that radiology will be useful in medicine and that we will avoid often extreme errors of interpretation into which one will inevitably fall without the solid medical education that should be demanded of all radiologists.

"The X-ray examination gives us on the screen or plate only shadows and light, white and black. It informs us of form, extent, position, degree of opacity of shadows and their relation with this and that organ. It says nothing of their nature, their anatomic value, their evolution. These facts which the X-ray man lacks can be given him by the doctor from a history, a study of the evolution of the disease, finally, and above all, by the use of other methods of exploration, auscultation, palpation, percussion, etc., etc.

"The wise interpretation can only be made with the aid of these facts, without neglecting any. It is a question of deciding

which of them are the most important and why; in short, after having made a careful and detailed analysis, one must make a synthesis. It is of this that diagnosis consists.

"One must not count on the X-ray examination to bring you a ready-made diagnosis, it will only bring you errors. This examination is only meant to furnish a series of facts, very useful and very different from those collected by other methods.

"In rare cases the X-ray examination can be decisive, and direct the examination in a totally new direction; in others it is limited to confirming the information already gathered. Even when a priori it appears useless, it should not be neglected, it is often then that it is most interesting and that it furnishes the doctor with the most unexpected facts.

"But it is always the doctor who must decide the last resort because diagnosis must not be a matter of impression, but a work of judgment, intelligence and labor. Impressionism is not defensible in medicine."

Regarded in this conservative way, the X-ray offers to medicine brilliant prospects of assistance in diagnosis. Regarded onesidedly and with over-enthusiasm, it can only make our work less careful and less reliable.

Asheville, N. C.,
May, 1917.

THE RŒNTGEN RAY IN THE DIAGNOSIS OF PNEUMONIA, PLEURAL DISEASES AND PULMONARY TUMORS.*

By MORRIS MANGES, M.D.,

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In bronchopneumonia the X-rays are of no value unless the areas of infiltration are so large and numerous that they have coalesced into large patches. The small scattered foci of the ordinary bronchopneumonia are too small to produce distinct shadows. It has been said that it may be difficult to differentiate these from the shadows observed in some cases of tuberculosis. Such statements are purely academic, since the history and course of the two diseases are usually too unlike to cause any such confusion.

In the cases of acute lobar pneumonia observed in hospital practice the X-rays are of relatively little value except at the beginning or in the later stages of the disease. In the fully developed disease the patients are as a rule too sick to lend themselves to much investigation, and this is limited to the use of plates only. Still, the X-ray examination by means of plates can be carried out in quite a number of patients; but the results will be unsatisfactory unless the observer is skilled and has a good apparatus, because of the rapid breathing of the pneumonia patient and his inability to hold his breath.

The indication for the use of the X-ray in the early stages before the diagnosis can be made by the physical signs exists in

* Read in the Symposium on the Rœntgen Diagnosis of Thoracic Diseases at the American Climatological and Clinical Association, Lakewood, May 31, 1917.

patients with unexplained fevers; or it may arise in the later stages when one suspects complications such as pleural effusion, empyema, interlobar abscess, abscess or gangrene of the lung. Serous or purulent effusions may often be recognized by exploration with the needle, and many may think that the X-rays are superfluous in these cases. This is by no means true and will be referred to later on. The X-rays are absolutely indispensable in the other complications which yield so few and misleading signs to the ordinary method of physical examination. Nowhere is this better shown than in interlobar abscess, in which the hanging shadow makes such a striking X-ray picture. Abscesses and gangrene do not give such clear shadows in the majority of cases. But in all of these three complications of pneumonia the X-ray examination may be life-saving to the patient in two ways: first, by making the diagnosis; second, by saving the patient the dangers and discomforts of the exploring needle. I know no field where more harm is done to patients than in the repeated blind explorations in these very sick patients. I will say nothing of the physical suffering; I would rather emphasize the great harm done to them by the infection of the pleura and by the spreading of the infection in the lung by these repeated blind punctures. I would go even further and say that no aspiration for suspected empyema should be made if it is possible to have a preliminary X-ray examination. Few physicians realize that the mortality of empyema is 28 per cent.; some part of this mortality may be laid to the needless trauma and infection of the exploring needle.

The topical diagnosis of these postpneumonic complications which the X-ray has made possible has also given us the new surgery of the lung in which such splendid work is now being done.

In acute lobar pneumonia the shadow is usually wedge-shaped with the apex at the hilus and the base toward the pleura. As it extends, the shadow is not homogeneous, as if there were various degrees in the development of the infiltration. The shadow is

not so dense as it is in fluid or in pulmonary tumors; it does not stand out so prominently from the tissue of the lung. Red and gray hepatization cannot be differentiated. These are Rieder's views. Steyrer, on the other hand, asserts that there is no peculiarity by means of which a pneumonia can be differentiated from other pulmonary lesions except by the rapidity of the change in size and form of the infiltrated areas. Hence the history must be compared with the findings. He also emphasizes the fact that pneumonia may occur in a tuberculous lung.

The X-ray studies of acute lobar pneumonia have yielded some very interesting results. They have demonstrated that the old idea of a central pneumonia which has been subjected to so much ridicule is the correct one, in spite of the presence of the crepitant râles which so many clinicians claim to arise in the pleura. No pneumonia begins at the periphery. It always begins at the hilus and extends toward the periphery. A few cases have been reported by Tendeloo, in which the whole lobe was involved at once.

The focus of infiltration may be localized by the X-ray within a few hours after the initial chill, *i. e.*, long before there are any physical signs to indicate the process. This is a very important observation which may be of great practical value, for the X-ray findings may often precede the definite physical signs by 36 hours or even more. Kraus and Rieder have even claimed that they have been able to demonstrate definite shadows at the hilus in the so-called one-day pneumonias in which the patients are practically over the pneumonia when the patch is discovered by the physical signs.

These observations have been strikingly confirmed by Whipple and his associates in the intensive studies which they have been conducting at the Presbyterian Hospital. The cases which were studied were the patients with unexplained rises of temperature after operation. These usually occurred forty-eight hours after the operation with little or no cough. There may be some pleural pain; but, if it is present, it is insignificant. There is

no great increase in the respiration rate; in fact there is very little to call one's attention to the lung at all. Definite physical signs of pneumonia as a rule do not appear until twenty-four hours or more will have elapsed; or they may not be found until the temperature will have dropped, which is often in twenty-four hours. The X-ray examination shows a wedge-shaped shadow which begins at the hilus and extends toward the pleura. This shadow can be detected very soon after the initial chill or rise of temperature. These X-ray findings are not found in all cases, but they are present often enough to make it worth while to X-ray the chest in all unexplained rises of temperature after operations.

Resolution usually proceeds from the hilus or it may advance from the periphery. There is no definite rule as to the length of time the shadow may persist after the patient has defervesced. In most cases it disappears promptly. In others it may persist for a long time showing that the infiltrated area has not returned to the normal. On the other hand, there are the cases in which the X-ray is normal even when there is a persistence of bronchial breathing and râles. It has also been noted that it is difficult to follow the lobar distribution of the pneumonic process. This is not to be wondered at when we recognize the technical difficulties and the fact that these sick patients make poor X-ray subjects.

Interlobar exudates will be referred to later on.

In the recognition of abscesses of the lungs the difficulties are often great. They may be detected by the irregular round or oval shadows which are somewhat darker than the surrounding infiltrated tissues; the centers are sometimes lighter owing to the contained air; sometimes a fluid level may be seen. The shadow is more diffuse and is less definitely contoured when the process is gangrenous rather than an abscess. In abscess there is a tendency to the formation of cavities, whereas in gangrene the process is that of a dying off of larger or smaller masses. In abscess the contours are much sharper; in gangrene they are

vague and the areas are larger. As already stated abscess shadows often have air or fluid in their centers. But all these details belong to the rœntgenologist rather than to the internist.

Much will depend on the character and density of the pneumonic shadow, in which the breaking down into abscess or gangrene takes place, and this will explain why the recognition of this process is easy in some cases and is very difficult or even impossible in others. In most of these patients frequent and repeated examinations may be required before any definite diagnosis can be made. These X-ray findings must always be studied in connection with the patient's condition, the kind of fever and the amount and character of the sputum. These are the decisive factors and not the physical signs. It cannot be repeated too often that the physical signs of abscess and gangrene of the lung are most misleading, since in most patients they are surprisingly few and ill defined. The reason why so many physicians fail to make a correct diagnosis in these patients is that they do not realize this fact. The topical diagnosis on which so much depends for the radical surgical treatment can only be made by the X-ray.

The development of adhesions and involvement of the diaphragm can only be determined by X-rays. The same is true in the differentiation of pneumonia from subdiaphragmatic lesions. Kraus has shown that the development of diaphragmatic pleurisy is more frequent than is generally assumed. All of the cases which he observed except one occurred on the left side. The diaphragm was unusually high and had no respiratory movement. This could be verified by inspection of the left chest, where no respiratory changes could be observed. This was to be expected from the fact that the patient desists from taking deep breaths on account of the severity of the pain. There was often a high tympanic note on percussion which may conceal the pneumonic process, this note being due to the high position of the diaphragm with its associated high position of the stomach and colon. There may be local pain and tenderness in the

lower left chest. The differential diagnosis from encapsulated pneumothorax, subphrenic abscess or acute dilatation of the stomach may be very difficult with the ordinary physical method of examination. With the X-ray, however, the diagnosis is easily and promptly made.

From the somewhat academic value of X-rays in the diagnosis of acute lobar pneumonia except in the early and late stages, we come to the much more important relation of the diagnosis of pleural conditions. As regards pleural effusions more definite and absolute information has probably been afforded us than in any other pathological conditions. We have had to unlearn many of our former views as to diagnosis and others have been verified. Of the latter the most striking is the correctness of Garland's S-shaped curve of the upper level of the effusion. The fluid level is not a straight line; it is curved, being highest in the axilla, and slopes toward the vertebral column and the sternum. It is level only under two conditions: first, when fluid is secreted very rapidly, and secondly, when there is air, even in the smallest quantity, in the pleural cavity. It is surprising how small a quantity of air may change the curved line of the ordinary effusion into the horizontal line of a hydropneumothorax. Fibrinous adhesions play a very important part in the contour of the upper level and cause it to assume the most varied shapes. They may also cause the fluid to be encapsulated in various parts of the pleural cavity. That percussion flatness does not always coincide with the upper level is shown by the X-ray, because some of the flatness may be due to thick fibrin or compressed or solidified lung.

Another misconception exists as to the rapidity of the change in the fluid level with change in the position of the patient. This takes place much more slowly than is generally assumed, unless air is present in the pleural cavity. In the latter condition the change takes place very rapidly. So, too, no diagnostic information can be gained from the density of the shadow as to the character of the fluid, for clear serum and thick pus give the same kind of shadows.

Kraus has called attention to the fact that the compressed lung above the fluid is not necessarily darker than the normal lung on the other side. On the contrary, it may even be less dense. Small exudates may at times be very difficult to discover on account of the shadow of the dome of the diaphragm. As a rule they collect in the pleural sinuses if there are no adhesions Holzknecht has advised the distension of the stomach if the suspected small effusions are on the left side. When the quantity of the effusion is 500 c.c. or more the shadow is usually typical. Any deviation from this indicates either that the exudate is of long standing or that there are adhesions. The position of the diaphragm is usually low, except when its movement is restricted by the presence of a diaphragmatic pleurisy. It is lowest in very large effusions or where the effusion is one of long standing.

The exudates which I have found most difficult to determine by X-ray examination are those which are associated with pneumonia. Here the combination of solid lung and fluid makes shadows which are often very difficult to interpret, so that faulty diagnoses are by no means uncommon. The physical signs would indicate fluid quite clearly, yet the X-ray plates would be equivocal. In not a few of these cases the aspirating needle has proven the presence of fluid.

Interlobar exudates produce "hanging shadows" which extend along the fissure line from the hilus toward the lateral thoracic wall. The size of the shadow does not always agree with the area of dulness, since much depends upon the distance of the exudate from the chest wall. So, too, the needle will not always find pus in all parts of the area indicated by the X-ray, because the dulness may have been partly due to compressed lung. Kraus emphasizes the fact that the upper border of the shadow is sharp, but that the lower is not so well defined on account of the presence of compressed lung. He also states that at times it may be difficult to differentiate the shadow of the interlobar exudate from that due to the infiltration of tuberculous glands in the hilus with an extension of the infiltration into the middle lobe.

The best results in the interpretation of fluids and exudates in the chest are to be obtained when the fluoroscopy is combined with the use of plates. If one depends upon either of these alone errors will frequently arise, and yet even when these are carefully combined mistakes may nevertheless occur. A striking instance of this occurred recently in a patient of mine where the X-ray examination and the exploring needle both agreed in making the diagnosis of a localized empyema. At the operation, however, no pus was found in the pleural cavity On the contrary, the lesion proved to be a small lung abscess. Similar instances must have occurred in the experience of everyone.

I must be pardoned if I again emphasize the fact that whereever it is possible to employ the X-ray in pleural effusions, especially when an empyema is suspected, an aspiration should *not* be made before the X-ray examination will have been made. Many needless aspirations may be avoided if this precaution is taken. Instead of going in blindly we can be guided as to where the needle is to be introduced, even if we are most skilled in auscultation and percussion. The very best results in empyema are obtained if we make a tentative diagnosis from the history of the case, the physical signs and the X-ray findings; then turn the case over to the surgeon and let him aspirate with the patient on the operating table, prepared to be operated on immediately if pus is found.

The failure of the old method of treating empyema is shown by its high mortality of 28 per cent. and the marked deformities and repeated operations and the extensive thoracoplasties in many of the survivors. As to what can be done to reduce this mortality and to prevent these deformities and extensive thoracoplasties I would refer to the excellent results which Lilienthal has obtained by his new methods in the early radical treatment by means of major thoracotomy. In this work the indications are largely guided by the information obtained by repeated X-ray examinations.

Concerning pneumothorax much need not be said because the findings of the X-ray and the old methods of examination usually agree. One point, however, deserves notice, and that is that small pneumothoraces are not infrequently found on X-ray examination where one has little or no reason to expect them. In the past few years I have observed this eight times. These small localized air shadows are usually found near the mediastinum and were not associated with any displacement of the thoracic viscera. It is difficult to explain these cases. The simplest view is to assume the rupture of air vesicles in a part of the pleura which has been shut off by adhesions. This would explain the air remaining localized and would also explain the absence of any visceral displacement. All of these patients had severe coughing spells and it would be easy to assume that these paroxysms caused the rupture of air vesicles. In none of them had any aspiration of the chest been performed.

The thoracic tumors in which we are interested in this symposium are those which are in the pleura and lung hilus and those which are associated with involvement of the mediastinum. The pleural tumors may be dismissed at once from consideration, since in nearly every instance they have massive effusions which constitute the only X-ray finding.

The tumors of the lung which are most frequently encountered are those due to syphilis, sarcoma, carcinoma, hypernephroma and Hodgkin's disease. Of these the Hodgkin's tumors give the most clearly defined shadows which are sharply contoured round areas; they are either single or multiple and may occur in any part of the lungs. These Hodgkin's tumors are usually only disclosed by the X-ray; they are found by accident as a rule, for they usually give rise to no physical signs and they are discovered in patients with unexplained fevers who are submitted to X-ray examination with the vague idea of being able to find something in the chest to establish a diagnosis. This of course does not apply to those cases with an established diagnosis of Hodgkin's disease, where the presence of respiratory symptoms

indicates X-ray examinations of the chest to discover infiltrations in the lung.

Syphilitic tumors usually show massive homogeneous shadows which are most commonly at the base, less frequently at the hilus. It is erroneous, however, to assume, as is usually done, that gummata are always at the base since they may occasionally occur in the upper lobe. The nature of the shadow will depend upon the changes which take place in the lung. Breaking down or cavities will give the characteristic pictures. If a gummatous process is associated with tuberculosis the pictures may be very bizarre and difficult of interpretation.

Sarcomata vary, being sometimes sharply rounded in contour and at other times they have irregular, ill-defined shadows. They may occur in any part of the lung and they may or may not have hilus infiltration.

In carcinomata the infiltrations are extremely variable in their distribution, contour and density. They are much more banded in contour with ill-defined borders. They always radiate from the hilus and are always associated with extensive infiltration in the mediastinum.

The proper interpretation of X-ray findings in pulmonary carcinoma is often extremely difficult and may baffle even the most expert reader of the screen and plates. This was recently well shown in two patients on my service in Mt. Sinai Hospital. Both patients were elderly men who had suffered from hemoptysis and were admitted to the hospital for this symptom. The first patient was a man 57 years old, who had a bloody expectoration for four months, which was at first mucoid and blood streaked; later it was pure blood. The physical signs were indefinite. Anteriorly on the right side of the chest there were impaired resonance to the third rib. There was moderate dulness in the interscapular space and some tympany was present at both apices. In the second right interspace there was a small area of broncho-vesicular breathing and whispered breath voice. Apparently there was no diminution of the amount of air entering

the right upper lobe. There were no râles, except at both bases. No tubercule bacilli were found on repeated examinations and the Wassermann test was also negative.

The X-ray examination showed coarse infiltration of the entire right upper lobe with broad coarse strands which radiated in all directions from the hilus. From the fluoroscopic plates, Dr. Wessler stated that he could not determine the nature of the infiltration. The diagnosis was cleared up by the bronchoscopy which was performed by Dr. Yankauer. He found a blood clot in the trachea which came from the right upper lobe and extended into the upper right bronchus. In this bronchus at a distance of 2 cm. from the trachea he found a bulging of the bronchial wall reducing the lumen to 1 cm. The upper half of this bulging was contoured like the bronchus; the lower half was smooth and softened. The stenosis looked like one which was extrabronchial. A piece of tissue which was removed was found to be carcinomatous.

The second patient came under my observation a few weeks later. He was a man, 63 years of age, who had suffered from cough and bloody expectoration for nine months. At the onset he raised a cup of blood but later the sputum had been mucoid and bloody, six to eight ounces being raised in twenty-four hours, mostly during the night. In the right lung dulness extended from the apex to the second rib; posteriorly the right apex was dulled. The breathing over the right apex was harsh. The physical signs over the rest of the lung were normal. A moderate number of râles was scattered over both chests. The only change which could be noted was that the right side did not move as freely as the left. The examinations for tubercle bacilli were negative, as was also the Wassermann test. No diagnosis was possible from these few vague signs. Dr. Wessler reported the following: "There is a dense homogeneous shadow with sharply curved outlines which extends from the right apex down to the level of the second space anteriorly. This shadow merges mesially with the shadow of the aorta, from which it can be

distinguished. On fluoroscopic examination there was little if any pulsation in the shadow which seems to occupy the entire right apex. The aorta is generally dilated. The heart is normal in size and on the right side shows pleuropericardiac adhesions extending from the right diaphragm from the border of the heart. It is not possible from the radioscopic examination to distinguish between a new growth and an aneurysm of the aorta. I am, however, inclined to favor a diagnosis of sacculated aneurysm of the ascending aorta."

This diagnosis of aneurysm was not justifiable from the clinical picture, as this pointed more toward a pulmonary neoplasm. A bronchoscopy was therefore performed by Dr. Yankauer. He found a stenosis of the right upper bronchus which was due to the bulging in of the anterior wall one half centimeter below the bifurcation. Two centimeters below this the walls almost met, leaving only a small chink for the lumen. The lower part of the trachea was ulcerated. A piece of tissue which was removed was found to be a squamous-celled carcinoma.

These two cases of bronchial stenosis due to carcinoma of the lung are most unusual and should not be used as a criterion. They are simply reported as showing the great difficulties which sometimes may be encountered in trying to establish a diagnosis of pulmonary carcinoma by means of the X-ray.

The X-ray diagnosis of malignant disease of the lung has an important practical significance, to which Lilienthal has called attention, for these deposits in the lungs may be metastases from malignant growths in distant parts of the body. "No limb should be amputated for sarcoma, no hypernephromatous kidney should be extirpated without rœntgenologic information about the condition of the lungs. If the characteristic discs of opacity are present it indicates a state in which palliation alone is justifiable. It is unfortunate that too few surgeons avail themselves of this valuable and informing aid to prognosis."

It is a matter of regret that the time at my disposal will not permit me to discuss the tumors of the mediastinum, as this is a

field the proper discussion of which would require more time than is available to me. I feel that I can devote this time to a better purpose if I close by referring to a subject of a more general nature.

In this symposium much will be said about the good which is being done through the introduction and general use of the X-ray in diagnosis. While agreeing with all this I should nevertheless like to refer to the harm which is being done by them. I refer to the neglect of the careful physical examinations which were the rule before the X-rays were introduced into medicine. Then we had no such easy recourse and the diagnosis had to be made by repeated careful physical examinations, a careful weighing of the history in all its features and a search for signs even of the most apparently insignificant nature. By a correlation of all these a correct diagnosis was often made. Now only too often a superficial examination is made and only casual attention is paid to the history, because so many, and I am sorry to say that I myself have been guilty of this, have preferred a resort to the alluringly easy X-ray examination of the chest rather than to the painstaking working out of details by the older methods. This neglect of the physical signs would not be any loss to us if the X-ray afforded infallible diagnoses; but until it does so it is the duty of all of us who are entrusted with the teaching of students to encourage the use of the old methods of physical examination, the searching for every feature of the diagnosis, the careful correlation of the histories with the symptoms and to make the X-ray examination play the rôle of being only a part of the physical examination and not the absolute and decisive one which is now so prevalent everywhere.

X-RAY DIAGNOSIS IN DISEASES OF THE CHEST.*

By GEORGE E. PFAHLER,

PHILADELPHIA.

THE value of the X-rays in diagnosis of diseases of the chest has been definitely established, and is being almost universally recognized. The X-ray will give evidence of disease within the chest: (1) Whenever there is any change in contour of the chest as a whole, or any part of the chest; (2) when there is any change in density of any portion of the chest, or any change in density of any organs within the chest; (3) evidence of disease will be demonstrable when there is any change in the movements of the organs within the chest.

Referring to the heart, the aorta, the diaphragm, or the lung tissue, the degree of value of this evidence will depend upon: (1) The skill of the operator in producing good plates, or in obtaining good fluoroscopic images upon the screen; (2) and by far of greater importance, will be the skill with which the abnormalities are interpreted. On this latter subject with regard to interpretation, one should have a knowledge of all the possible macroscopical pathological changes which may affect any organ within the chest or the chest as a whole. Therefore, the more the rœntgenologist knows of pathology, the more skillful is he likely to be in the interpretation of the evidence at hand. (3) One should have a thorough knowledge of the history and progression of the various diseases affecting the organs within or about the chest so that the proper value can be placed upon the evidence at hand according to the probable stage of development in any particular disease. Therefore, the more the rœntgenologist

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knows of the diseases of the chest in general, their histories, their favorite locations, the more valuable will be the evidence obtained by the rœntgen rays. While I believe that the most valuable and most definite evidence of disease can be obtained by the rœntgen rays, it should not be considered as an isolated factor of diagnosis, but must be measured in conjunction with the history, physical signs, and the knowledge of the progression of the disease in every individual case. Inconsistency or contradiction in evidence must be considered most carefully, and so far as possible be eliminated before arriving at a final diagnosis. Inconsistencies and contradictions usually mean ignorance in some respect, and a frank acknowledgment of this fact to one's self will often lead to increased effort, further study, and a proper diagnosis. It is my observation that very generally mistakes in diagnosis are not so much the result of ignorance as of insufficient work or carelessness. In this very brief paper, it will be impossible to present any phase of this subject exhaustively. One would require a hundred, if not a thousand times more time and work. Your President, therefore, has requested that I present merely a resumé of facts as observed by rœntgenologists.

Pulmonary Tuberculosis.—Probably the most frequent use of the Roentgen Rays and the most valuable evidence of disease within the chest is obtained in the study of tuberculosis. Tuberculosis can be recognized as early as there are tissue changes which will produce some variation in the degree of density of the lung. The *early* X-ray evidence is obtained only upon plates, and these plates should be made stereoscopically—preferably a pair of stereoscopic plates made anteriorly, and a pair posteriorly—and added to this there should be a special plate made of the apices of the lungs of 8×10 size, and the rays sent obliquely downwards in line with the chin so as to project the clavicles downwards and clear the apices of the lungs. The evidence of disease about the apices of the lungs can be further demonstrated more clearly if one trains the patient to forcibly exhale against resistance. That is, if the patient takes an ordinary deep breath and then

forces the air against the lips and palate without allowing any air to escape, the apices of the lungs become well distended. This fact can be seen fluoroscopically in a healthy individual. If one watches the respiratory act during inspiration and during full retention of this inspiration the lower portion of the lung becomes greatly distended, but the apices are less distended. During exhalation, however, under normal conditions, the apices of the lungs become distended with air, and become beautifully illuminated. When good stereoscopic plates have been made showing a great amount of detail, and one studies this under favorable conditions, the evidence of early tuberculosis will consist in the minute deposit of tubercles in the location where pathologists have long determined to be the most frequent points, and, while conditions vary, and while there are exceptions to all rules, one finds most frequently this evidence at the apices and particularly the inner portion of the apices of the lungs extending down along the vertebral border, but this evidence may occupy also the apices of the lower lobe, or the outer portion of the middle lobe in adults, while in children the disease seems to most frequently extend outwards from the roots of the lungs.

Tuberculosis in children is best studied by means of perfectly good intensifying screens upon plates because of the difficulty in getting the patient to hold still even for an instant. If one uses intensifying screens, and a soft tube, one can obtain great detail with very short exposures. A tenth to a twentieth of a second exposure in very young children will usually be sufficient. In looking for early tuberculosis in adults it is well to keep in mind the "fan-shaped" arrangement upon which evidence Dunham lays so much stress. This "fan-shaped" arrangement is a mere matter of seeing the disease in the distribution of the terminal bronchi, and one must not lay too much stress upon this arrangement, for when it is present it is of value and should be looked for, but one must keep in mind that the fan-arrangement or the triangular arrangement may not always be presented with the flat side toward the eye, and one may see the grouping

on edge. If one keeps in mind simply the terminal branches of the bronchi and looks for the lesion in such an arrangement, one can often make the diagnosis quite early. This evidence, of course, should not be taken as an isolated factor, but in conjunction with the physical signs, and, in many instances, it will make a doubtful diagnosis positive, or may be the means of eliminating the probability of tuberculosis.

Advanced tuberculosis can, of course, be recognized by everyone if sufficiently searched for, and it can be recognized by all methods of diagnosis. In fact, very commonly the patient brings the diagnosis to the physician.

Extensive consolidation and cavity formation can be demonstrated clearly on plates or can be seen fluoroscopically, and the extension of the disease from the apices downwards toward the base of the lungs should always make one think seriously of tuberculosis, and this diagnosis should be definitely proven negative before given up. The infiltration which one so frequently finds extending into the lower lobe from the lower bronchial tree is, I believe, very rarely tuberculosis, but is found commonly in chronic bronchitis. It is due to infiltration and is usually secondary to dust infection. Bronchiectasis must be considered in these conditions especially.

The three diseases which are the most difficult for me to differentiate from early tuberculosis are metastatic carcinoma, leukemia and syphilis. Very generally for the differentiation of these conditions one must study very carefully the history and progression of the disease, for, of course, it is entirely possible to have any of these three diseases associated with tuberculosis. There are, however, differentiations which are of some value

First, Metastatic Carcinoma.—The history of previous carcinoma, particularly when it has involved the breast, will be of considerable value. The disease itself may be distributed through the lungs in a manner almost identical with tuberculosis. One may have a general diffused infiltration by carcinomatous nodules and give almost identical evidence with the general or

disseminated tuberculosis, or one may have it only occupying the apices of the lungs just as in tuberculosis. Generally, however, one finds the disease spreading outwards from the roots of the lungs or the mediastinum. It may, however, occupy any portion of the lungs and extend from this central source. There is this slight difference: In metastatic carcinoma the disease develops from some point and gradually invades the surrounding tissue by direct extension, presenting I believe a more uniform appearance than is true with tuberculosis—in other words, a less mottled appearance. For instance, if disease begins in the apices one has at the time, of course, an increased density very gradually shading into the surrounding tissues, and if one follows the disease over a few months the progression can be observed very positively and the increase in density both in the original portion as well as in the extension outward. There is, I believe, very rarely, if ever, cavity formation. This disease is, I believe, more generally associated, too, with pain.

Second, Leukemia presents a general mottling very similar to that of tuberculosis, but it is more apt to be a general affection of the lungs. This differential diagnosis may seem unimportant to the average physician; however, during the past year I have had a patient who had been confined for six months in a tuberculosis sanitarium because she had a persistent cough, a moderate degree of anemia, and physical signs suggesting tuberculosis of the lungs. A blood count had been made, but the differential count had been omitted. The chest had been examined but a very large spleen and the abdomen had not been palpated. It shows that this is carelessness, but so are most mistakes in diagnosis.

Third, Syphilis of the lungs, I believe, is rare, but it does occur, and there are physical signs and symptoms similar to tuberculosis, and rœntgenographically the appearances of the lungs may resemble tuberculosis very closely. Here again, however, the involvement of the lungs is more apt to be of a general character, or to extend outwards from the roots of the lungs

rather than to involve the apices of the lungs or the apices of the lobe and extend downwards.

Bronchiectasis must also be differentiated from tuberculosis. In its classical form the diagnosis is, of course, easy, but many cases occur in which the symptoms are in no sense those of the classical type which we have studied in our student days. When the cavity or cavities are large, and when large quantities of sputum are expectorated, which the student is taught to expect, the diagnosis is easy, but there are quite a large number of patients who have a persistent cough, a moderate degree of emaciation, considerable expectoration, in which none of these classical symptoms are present, and they refer to the type in which there are many very minute bronchial dilatations, as can be shown by stereoscopic observation. They give physical signs very similar to tuberculosis, but rœntgenographically the disease is distributed more especially to the base of the lungs and above the lower bronchial trees, though it may involve even the upper bronchi.

Broncho-pneumonia cannot always be differentiated from tuberculosis, and especially is this true in children, in whom there may be the same onset, the same general symptoms, and, as a matter of fact, practically the same character of rœntgen findings. In most cases, and in children especially, the disease extends outwards from the roots of the lungs. It has seemed to me, however, that when one studies the plates carefully in the tubercular variety, there seems to be more of the tendency for the disease to extend upwards about the upper bronchial tree, while in the other form of bronchial pneumonia the disease seems to have the tendency to extend outwards and downwards from the roots of the lungs. In adults, the broncho-pneumonic type of tuberculosis is less common and the possibilities, therefore, are in favor of an infectious bronchial pneumonia rather than tuberculosis when the disease can be clearly demonstrated as extending from the roots of the lungs outwards.

Lobar Pneumonia.—The rœntgen evidence of lobar pneu-

monia is quite clear, and generally this method of diagnosis is not called into play. Occasionally, however, the symptoms and physical signs are obscure, and in such instances the rœntgen diagnosis will be of value. Studied rœntgenographically lobar pneumonia occupies usually one of the lobes of the lungs, sometimes two or more. Rarely is it confined entirely to the lobe, and very commonly there is not total consolidation. It is probably this fact that leads to obscure physical signs. When there is much consolidation about the root of the lung with a peripheral compensatory emphysema the physical signs would naturally be obscured, and in such instances a rœntgen study will be of the greatest value. The rœntgen rays will also be of value in studying the case during convalescence. It usually requires a considerable time—sometimes weeks or a month—for the lungs to completely clear up after a lobar pneumonia, and, as is well known, such affection is associated with tuberculosis, or may pass imperceptibly into tuberculosis, which is probably due to the fact that there has been a previous infection by tuberculosis and the super-added pneumonia breaks down the resistance sufficient for the disease to develop.

Tumors of the Lungs.—Practically all tumors of the lungs are secondary, and therefore one can very generally obtain a history of previous involvement of some other portion of the body by tumor tissue. However, one may have primary carcinoma of the lungs. I have had the opportunity of studying and treating one such case The rœntgen evidence was that of a small area of consolidation extending outwards from the roots of the lungs. The diagnosis was definitely made by the patient, who was a physician, coughing up a portion of the tissue, which was examined microscopically. This disappeared under X-ray treatment. In the study of tumors of the lungs one very generally has a previous history of the removal of malignant disease, but very often there is a vague history of the removal from some other portion of the body of a tumor, the diagnosis of which was uncertain or perhaps considered benign. Therefore, one must

not hastily jump at conclusions as to the diagnosis of metastatic disease of the lung.

Metastatic Sarcoma.—In my experience sarcoma is a frequent occurrence in the lung tissue secondarily, when sarcoma has been present in some other part of the body. It gives characteristically sharply defined nodules scattered through the lungs. The patient may have even no pulmonary symptoms or only a very slight cough. I have found extensive involvement of the lungs when there were absolutely no pulmonary symptoms, and it is my practice, when there is a history of sarcoma, and perhaps a recurrence, to always examine the lungs for metastasis. Very frequently such disease will be found. I have never seen metastatic carcinoma present lesions that were so sharply defined. Very generally the carcinomatous involvement of the lungs extends by the gradual infiltration, and when it occurs as isolated deposits, surrounded by healthy lung tissue, there is still this general fuzzy extension.

Affections of the Pleura.—*Acute pleurisy* can probably not be diagnosed by the rœntgen rays excepting insofar as it can be seen to limit the motion of the portion of the chest involved, particularly as it affects the movements of the diaphragm of the affected side. *Chronic pleurisy* is very generally associated with adhesions. It will, therefore, when it involves the diaphragm, give very characteristic interference with movement of the diaphragm so that at each point of adhesion of the diaphragm there will be a limitation of the movement, and sometimes this gives a puckered-up appearance of the diaphragm at a number of points. If the pleura is greatly thickened, no matter what area is involved, it will also give some decrease in the transparency as compared with the surrounding areas of the lungs. If of the tubercular variety, which is very common, and occupies the apices of the lungs, there is very frequently associated tuberculosis of the lungs, and one obtains a general opacity or indefiniteness in outline, and this very frequently involves the apices of the lungs. *Pleuritic effusions* should always be examined with

the patient in the upright position. In the recumbent posture one obtains little or no evidence unless the effusion is very great. When the patient is examined in the upright position with pleural effusions, one can recognize an upper level of increased opacity. This is not great, and not usually sufficient to obliterate lung detail, but a distinct line of demarcation can be recognized between the clear lung tissue and the effusion below. This line is never level in the sense that one obtains a level of water in a basin, but is curved, and usually curved outwards and upwards. This evidence is obscure when one has associated disease of the lung, and is not recognized, therefore, in an early stage. In advanced pleural effusion there may be considerable compression of the lungs, in which instance the disease is more clearly recognized. The character of the fluid can rarely be differentiated by the rœntgen rays. In a general way empyema gives slightly greater increase in density, but this is not of sufficient value to be seriously considered.

Encysted Pleural Effusions or Encysted Empyema.—For the diagnosis of this condition the rœntgen studies are of great value, and in this one should make both a fluoroscopic and a rœntgenographic study. Appearances in this condition are usually very characteristic. There is a dense area surrounded by clear lung tissue and the lines are sharply defined. In this respect the disease differs from abscess of the lungs.

Abscess of the Lungs.—In the early stage abscess of the lung will present simply the appearances of a localized area of consolidation shading gradually into the surrounding tissues. After there has been necrosis of the center, and evacuation of a portion of the abscess, one will obtain the appearances of a cavity surrounded by consolidation progressively shading into the surrounding tissue. This abscess may be pyogenic in origin, or it may be tubercular. If tubercular, one will have associated the other evidences of tuberculosis previously referred to. If pyogenic, the disease is likely to occupy the central portion of the lung or the base of the lung. For this purpose the rœntgen

rays will be also of considerable value in locating the abscess definitely before surgical intervention, if considered desirable. When an abscess heals spontaneously it has been my observation that the diaphragm on the affected side rises progressively, permitting the cavity to close and then, after healing has taken place, there is a gradual increase in the movements of the lung and diaphragm until it becomes quite transparent again.

Gangrene of the lung I believe differs in no respects from abscess of the lung rœntgenographically, and the real differentiation in abscess of the lung and that of gangrene consists in the use of the olfactory sense, on the part of the rœntgenologist or attending physician.

The Heart and Blood Vessels.—In the study of the heart one observes the size, position, shape and peristaltic contractions, and the variation of these will often give most valuable evidence for diagnosis.

Size.—The size of the heart will, of course, vary with the individual, and one cannot set any definite standard of the size of the heart. The size of the heart will vary just as the size of any other organ of the body will vary, whether it be the stomach, the nose, or the little finger. For none of these can one set a definite standard. The size will vary not only with the individual, according to birth, but also according to the occupation and the use that he has made of his heart and body, so that the mere record of the size of the heart, without due consideration of all other factors, is of little real value. Therefore, extensive use of the orthodiagraph has never appealed to me. When there is considerable variation in size this observation will have considerable value. The size of the heart can be accurately determined by means of the teleo-rœntgenography, which consists in photographing the heart at a distance of about six feet or two meters. This gives almost exactly the size of the heart, there being practically no distortion. Such a record is infinitely more accurate than any that can be obtained by percussion or "physical diagnosis." In fact, I believe that by percussion one cannot

determine the size of the heart, as a rule, more accurately than within one half inch. This statement will startle those who have not had actual experience with expert diagnosticians measured up against photographic records.

The Position of the Heart is also variable, and will vary with every movement of the diaphragm. When the diaphragm is high, either from forced exhalation or from distention of the abdomen by tumor, fluid or gas, the heart will occupy the highest position and the apex of the heart will be projected upwards toward the left side of the chest. In other words, the heart will assume more of a transverse position. During forced inspiration on the other hand, the heart will assume more of a vertical position. In addition to this, in most movements, there is more or less rotation. Therefore, in interpreting the size and position of the heart one must take into consideration the position of the diaphragm and the associated abdominal conditions.

The Shape or Outline of the Heart I believe is of the greatets diagnostic value. One can recognize hypertrophy or dilatation in the region of any of the cavities of the heart, and, as an example, when one has hypertrophy of the left ventricle there is distinct projection to the left and distinct enlargement in the region of the left ventricle. With general dilatation, on the other hand, the heart changes from its pear shape to a more or less globular form, and the variations between these two extremes can be recognized definitely, and if properly interpreted will be of considerable value in the ultimate diagnosis.

The Peristaltic Movements are perhaps of even greater value, and these must be observed fluoroscopically or upon the screen. There is a striking difference between the strong, steady contraction of the healthy individual as compared with the rapid, flabby and more or less general wavy pulsation associated with general dilatation.

The study of these various points with reference to the heart is ultimately of considerable value not only in arriving at a diagnosis, but in estimating the progress of the disease either towards recovery or towards ultimate death.

Pericardial Effusion can be recognized and often diagnosed by means of the rœntgen rays. In this one finds, as would be expected, a more or less triangular sack with a base at the bottom. The general shape of the heart and pericardium is a large triangle, and if the effusion is great one may get a total absence of the peristalsis, or if not great the peristaltic wave is definitely diffused or made less distinct.

The study of the *great vessels* is of much value. By making use of the oblique position one can recognize very early a general atheroma or localized plates within the aorta. This condition is commonly associated with angina pectoris so that when a patient has anginoid pains it is quite worth while to make such a study and establish more definitely the diagnosis.

Aneurysm is recognized by the tumor formation along the course of the aorta presenting expansile pulsation. These are the essential characteristics.

Mediastinal Tumors may occupy a similar position. They present no expansile pulsation, though they may present some transmitted pulsation. If the mediastinal tumors are multiple, such as commonly occurs with Hodgkin's disease, or other glandular conditions, one can very easily differentiate from aneurysm. If there is a primary growth such as sarcoma of the lung, one very generally recognizes the persistent extension outwards from the mediastinum into the lung tissue which clearly makes a differential diagnosis.

Conclusions.

1. The rœntgen rays are valuable in diagnosing any condition which will cause an increase or a decrease in density of the lung tissue.

2. The rœntgen rays will be useful in recognizing the position, fixation, or deformities of the curve of the diaphragm.

3. The size, position, outline and character of peristaltic movements can be recognized with regard to the heart and the rœntgen rays will, therefore, give valuable evidence in the

diagnosis of any condition which modifies any one of these factors.

4. Aneurysms and all tumor formations within the mediastinum can be recognized and usually definitely diagnosed.

DISCUSSION.

————: Dr. Manges made the statement that in bronchial pneumonia there is no evidence. I disagree with this. I think that just as soon as there is a change in the lungs, with change of tonicity, we see very decided signs of bronchial pneumonia. Particularly in children you can watch the bronchial pneumonia develop and study its course.

In regard to a point brought out by Dr. Pfahler (?)—perhaps I misunderstood him to say that the adhesions to the diaphragm were mostly on the right side. We certainly see adhesions oftener on the right side, but I think that is because the diaphragm presents more surface for study on the right side. On the left side it is covered by the heart. But pathologists will tell you that adhesions occur on both sides. You can bring this out by the curve if you rotate the patient, and there is no chest condition in which you cannot get additional information by means of fluoroscopic plates.

Taking up Dr. Minor's paper, with regard to the comparative value of ordinary physical signs and X-ray studies, the comparison all depends upon the skill on either side and the point of view. Both are of value and both should be used. I would say with Dr. Minor that if you must depend upon one alone, unquestionable skilled physical diagnosis is of the greatest value; and I feel too, as does Dr. Minor, that medical students are learning to depend upon X-rays and X-ray plates for diagnosis more than the practitioners—for, as Dr. Minor says, the X-ray is only occasionally available. You can't pick up an X-ray machine and go into the home of a patient. You have got to depend upon the physical signs. But that need not deprive you of any additional aid you can get. Some of us must walk, but if we can ride in an automobile and get to a place that much sooner why not ride? So let us make use of all the physical signs we have. Let us add fluoroscopy if we can, and it will be of value in proportion to the skill we develop in the use of it, and it is most important to interpret the value of everything we observe. Dr. Miner brought up the question of stereoscopic plates. I should say use a fluoroscope if you can—if you can add flat plates and cannot add stereoscopic plates by all means add flat plates. They will

be of undoubted value in confirming the physical signs, and they unquestionably record the amount of increased tonicity of any particular point in the lungs. Let us take the right apex as an example—if that whole area is tense it is up to you to decide whether it is due to an overlying thick muscle or tissue or fat, or whatever else it is that may be producing the opacity of the lung tissue. The interpretation is up to you—it is all there on the plate—it is a question of seeing and interpreting. When you use stereoscopic plates you have a little more opportunity of viewing the thing in perspective and getting the relative value of the particular shadow.

In regard to the heart work that has been so beautifully presented and which we are of course familiar with through the very excellent work presented in ————'s book (?), Dr. Pratt seems surprised that there are not more orthodiagraphs in use in America. I know of several in America that have long ago been thrown into the junk pile. I was much interested in the orthodiagraph when it was first shown at the World's Fair at St. Louis, and a group of rœntgenologists went over it pretty thoroughly. But first of all let us get down to primary principles: there isn't a man in this room who can tell me how big my little finger should be, and yet it is an easy thing to measure; there isn't a man in this room who can tell me how big my nose should be. And if you can't tell how big my little finger or how big my nose should be, how in the world are you going to tell how big my heart should be? Let us consider what influences the size of the heart. What you all know as clinicians is that there are many factors that come into play in this connection. In the first place, occupation and previous training. Take the student who goes through a course of strenuous athletics—he will have a different shape of heart than another, and yet his heart is normal—and from that extreme example down to the man who simply lounges around a hotel corridor, it is all a question of occupation, of the character of breathing and hereditary influences. There are many factors that enter into the question of the size of the heart, and when I get all through and try to sift out the evidence and determine what is of value, I would rather take the old practitioner who put his finger on your pulse and said, "Doctor, your pulse is all right." I would rather depend upon him to get me well than on the man who sifts through all this mass of evidence. I do not say that the orthodiagraph is not of value, but don't be misled by the mere size of the heart.

In regard to the telerœntgenograph—that is, making the plate at a distance of 2 meters or about 7 feet from the patient. There you have a permanent and exact record, and you have eliminated the personal factor. It has been shown by careful study that the personal factor enters into the making even of the dotted line or

markings with a pencil by means of the orthodiagraph—and three or four men making tracings of the same patient do not get the same record. So let us not be carried away by that sort of evidence.

The shape of the heart and the size of the heart, so far as you can determine them by the orthodiagraph, or by plates for that matter, are greatly influenced by the position of the diaphragm, as Dr. Pratt has so beautifully shown. And any distension of the abdomen or varying conditions in the abdomen will make a change in the position of the diaphragm. A full meal with increased distension crowds the diaphragm up, which accounts for the shortness of breath after a full meal; and the position of the diaphragm is also influenced by all other varying conditions—a fat abdomen pushes the diaphragm up higher than its position in a thin individual, etc. And unless the clinician or rœntgenologist who is presenting the evidence takes into account all these varying conditions he is not going to get much of real clinical value.

In regard to the shape of the heart I regard as of the utmost importance the points brought out by Dr. Pratt. The shape can be pretty well recorded on a telerœntgenograph. But I believe that even of more importance is the study of the peristaltic wave. A man who is trained and whose routine it is to study the healthy heart as well as the diseased heart will know the value of the strength of the heart beat, and by carefully adjusting your fluoroscopic work you can get at that evidence by the study of the heart fluoroscopically.

Dr. LANDIS. It is rather interesting that the late W. W. Gerhard, who was one of the most distinguished clinicians in this country, in writing about Laennec's discovery said that in the years immediately following there was a very strong tendency on the part of everyone to disregard all previous methods and confine themselves exclusively to physical diagnosis. And then some years later when the tubercle bacillus was discovered many men were constantly neglecting the signs in the chest and were examining the sputum exclusively. More recently, because of its extraordinary simplicity the von Piquet test has been insisted upon as a ready and easy method of determining whether or not an individual has tuberculosis. And now we come to the introduction of the rœntgen ray, and unless my experience is uncommon there are many men who are acting on suspicion and without any clinical effort to find out whether or not the individual has tuberculosis. They have plates made, and accept the dictum of the rœntgenologist. So that the rœntgenologist has had forced on him the position which I think in many cases he has accepted very willingly. He gives a perfectly ready-made diagnosis of what he thinks is present, and curiously enough that evidence is upheld and the plate accepted by many as prima facie evidence.

There are many who fail to recognize the fact that it is as yet, to use a sporting term, a fifty-fifty proposition. There will be instances where the X-ray man is very far wrong, just as the clinician is sometimes going to be far wrong. Recently I examined a man who had been seen by five men in different states. The signs were all markedly pronounced, and all agreed about the meaning of them. The apex beat of the heart was displaced to the left and there was diffuse pulsation; the breath sounds were much suppressed, and there were a few râles at the left base. We had stereoscopic plates made by a competent rœntgenologist, and got a report that the heart was enlarged, but that otherwise the chest was absolutely normal. Of course the question is whether we are going to take the opinion of the five men in regard to the physical signs or accept the evidence of the plate, which shows nothing.

I think the real value of the X-ray lies in the early cases and not in the advanced or moderately advanced cases, because by the time a patient gets to these stages the other evidences are very plain. When it comes to some of the early changes there are a number of conditions which may resemble tuberculosis. Undoubtedly any one who has seen that extraordinary collection of mycotic lungs in the Rush Pathological Museum must have been struck with the pitfalls that one can fall into both from the standpoint of the physical signs and the rœntgenographic studies. Even with the gross specimen in your hand I suppose that anybody, aside from the man who has had much experience with that particular type of infection case, would be very apt to mistake it for tuberculosis.

Then there is that extraordinary disease, coccidioidal granulous, which resembles tuberculosis so completely that one can hardly tell the difference between the two. It is the organism which you find in the sputum which gives you the clue. At present I have a woman under my care who has every symptom of tuberculosis; the physical signs are typical of tuberculosis at the left apex, and the stereoscopic plates are those of tuberculosis; and my colleague, Paul Lewis, for the last three years has been studying the sputum, which is full of small yellow granules, and yet up to the present time he has been unable to determine the organism which is causing the trouble. What form of yeast infection it is we cannot say. In this case no one has ever found tubercle bacilli during the last eight years. Another very common condition that I have been studying for the last six or eight months in association with Henry Pancoast, of the University of Pennsylvania, is changes in the lungs due to the inhalation of inorganic dust; and here again, from the standpoint of the physical signs and X-ray plates, the findings are almost identical with those of tuberculosis. The dust has a strong tendency to gravitate to the right lung and right apex, the result being that some of the earliest changes

consist in the thickening of the bronchial tree toward the right apex, and in the white, snow-flaky appearance, which becomes more marked later on. After some experience you can begin to distinguish between that condition and tuberculosis. As the ultimate result there is a widespread fibrosis with dilatation of the bronchi at the apex. The physical signs and the rœntgenographic plates are almost identical with those of tuberculosis; and I will defy any rœntgenologist who has had no experience with that particular form of pulmonary change to say from the plates that the changes are not due to tuberculosis. It is only when you take into consideration the etiology, the occupation and the course of this particular condition that it can be recognized.

The whole question has been very well put by perhaps the greatest clinician this country has produced, that is, the late Austin Flint, who said that the men who based their entire opinion upon one method are most certainly going to get into trouble. You have got to give very serious consideration to the history, the course of the disease and the symptoms from which the patient has suffered. You have got to have a very distinct knowledge and picture of the pathologic process which takes place in the lungs—what it does, what sort of lesions it produces, where they have a tendency to localize, and whether they have a tendency to spread from one point to another, and lastly, of the physical signs. In the matter of the interpretation of many of these changes, I am free to state that some of my own friends among the rœntgenologists have receded very strongly during the last year or two from the position they used to take. There was a time, I think, when they had invented a pathology of their own; but that position has been abandoned to some extent, and I feel that they are in exactly the same position with regard to the interpretation of many of these changes that the physical diagnostician was fifty or sixty years ago. I believe that what is needed beyond all other things at the present time is a close association of the rœntgenologist, the physical diagnostician, the internist and the pathologist, and if after a few years of study you have only a few cases in which the conditions can be traced out and proved to the end, you will have accomplished something. I think that at the present day there is too much guess work, and I am fully convinced that there is going to be a recession on both sides.

Dr. MILLER: I think we have been very fortunate in having a conservative, rational discussion of this matter from various viewpoints, which I believe appeals to all of us as very well meeting the situation as it has come to our own experience; so that I feel I have little to add except perhaps to emphasize one or two points. There can be no doubt at all, as Dr. Landis has well pointed out, that we

are bound to go through this phase with the rœntgen ray. We have done it with all sorts of methods; we have only very recently come through it in the laboratory. We are going to pass through it and reach a point where we shall know where we stand

There are two or three particular points I would mention in connection with some of the matters spoken of. First, in regard to Dr. Manges's paper, which I think was particularly good and which appealed to me because it touched on some of the problems I have met with in differential diagnoses and which will be very helpful to those who see cases of pulmonary tuberculosis. I want to ask whether, as I understand it, at the Presbyterian Hospital they have come to the point where after making the original study they make a routine X-ray examination of all patients, and find that they get some slight changes in practically every case.

The second point I was rather surprised to hear discussed, because undoubtedly Wilcox (?) has shown so well that bronchial pneumonia in children may be diagnosed early, and dissemination of the bronchial in infants can be watched very beautifully by the X-ray.

The third point, which I think is very important, is that of interlobar pleurisy. Interlobar pleurisy and serous fluid, from the standpoint of differential diagnosis. are frequently confused with pulmonary tuberculosis. The physical signs are very similar, and we often get signs in the pleura in association with râles and changes in the breath sounds and the X-ray there does help very beautifully in the differential diagnosis.

Fourth, the question of tumors. I have had two or three cases which have been very interesting. One, a woman of middle age who had had one breast removed sixteen years previously. She had been in perfect health since, except for a cough with no expectoration for two or three months. She was very much interested in the antituberculosis campaign, and came to me because she wondered whether she could have tuberculosis. She had no signs in her chest except a few sounds of bronchitis which came and went, but the X-ray at that time showed in the first place an enlargement of the glands at the root, so that in each lung there were about eight or ten very small nodules with sharp lines of demarcation but with a little fluffiness of the outlines, which were quite different from those you would expect in tuberculosis. The density did not seem to me particularly great in these masses, and while they looked practically round, there was a little haziness of the outlines. I had the opportunity of watching her when the stereoscopic plates were taken. She finally died in the course of six or eight months, and unfortunately no autopsy was obtained. After all, might the changes have been metastatic changes in the lung, as in Hodgkin's disease? I wondered what the Doctor

would say in regard to metastases, because it struck me as very interesting.

When we come to the question of the X-ray in pulmonary tuberculosis I agree practically entirely with what Dr. Landis has said. I don't know whether I agree with what he really thinks, because in discussing the question he does not go as far as I would as to the value of the X-ray and the physical signs. I think it is undoubtedly true that we do occasionally—though very seldom—see cases in which there are practically no physical signs whatsoever and in which the X-ray shows very extensive ones. But I am sure that when you come to advanced or moderately advanced tuberculosis there is a use for the X-ray and particularly for stereoscopic plates in order to have a permanent record. I think we all recognize the fact that by the physical signs we are able to make our diagnosis, and that the physical diagnosis is undoubtedly of much more value than the X-ray findings. But if you are watching a case over two or three years, a case with a little fever, a simple passing occurrence and incidental infection, the X-ray will often help you very materially. I think we have in these cases a use for the X-ray. Otherwise I might say that I agree entirely with Dr. Landis about the position of the rœntgenologist. I believe the clinicians are entirely to blame for the deplorable state of affairs that now exists. We have forced the X-ray man to the point where we put it up to him to send us a report which will make a diagnosis. But if my experience is similar to that of others, the X-ray men are only too anxious to get in touch with clinical men who are willing to study both sides of this question. Our rœntgenologist at the Bellevue Hospital said to me: "I wish more of the men interested in these problems would come over and talk these things over as you do." I said: "I don't care for your report until we get everything together. I don't care to have you make a diagnosis. The clinical aspects of a case of consolidation, of pneumonia with adhesions, or of bronchitis vary much (?) and," I said, "it is perfectly foolish for you to say there isn't a cavity there. You put yourself on record." He said: "I know that—I write out a long report and I simply describe what I see, and in one instance the surgeon said to me, 'What the devil do you mean by sending in a report like this? I want to know what the diagnosis is and what to operate for.'" So until we can work together we are ourselves very much at fault.

One other point—I believe that in many departments of clinical medicine in connection with the X-ray the rœntgenologist is going to be crowded out. I believe it is going to be more and more the clinicians who will study their cases with the X-ray themselves. I believe that until this we are not going to achieve the greatest possibilities of these combined studies—just as in many Wassermann

examinations we want to examine the specimen ourselves. Those who expect to be leaders in clinical medicine have got to do this work themselves, and until they do they are going to be misled. And we will gradually be saved from situations like the one in which I found myself about a year ago, when a person of moderate means who had had a careful study of the whole gastric tract came to me lamenting over a bill of $500 for the rœntgenological study. I said the charge was excessive for that sort of work, but the rœntgenologist said: "It is not—it is a question of my opinion, and I consider that my opinion given in that report is worth the same compensation as an exploratory laparotomy."

Dr. POTTENGER: I have been highly delighted with the discussions this morning. My own opinion is about as has been expressed by Dr. Landis and Dr. Miller. I believe that when it comes to the relative value of a physical diagnosis and X-ray plates the physical diagnosis by an expert takes first place. I think too that the average plate which is being taken and on which the diagnoses are dependent in this country is deplorable. It is worse than a poor physical diagnosis. If we are going to make progress in clinical diagnosis, we must get back to first principles and approach tuberculosis from a different standpoint. We must approach it as being a chronic inflammatory infectious disease which affects one of the important organs of the body, and we must study the physiological relationships of this organ with other organs and other tissues. These relationships are influenced by the visceral nerves. The lungs are supplied by two opposing nervous systems,—the sympathetic and the vagus. During the periods of toxemia, the sympathetics are stimulated centrally, which results in a hypo function of nearly all the secretory glands of the body. It also results in a decreased motility in the smooth muscles supplying the organs of digestion and respiration. As a result of the inflammatory process in the lung itself, there is a stimulation of both the sympathetic and vagus nerve endings in the pulmonary tissue, and these express themselves in two different ways. Stimulation of the sympathetic nerves expresses itself in those superficial structures of the body (skin, subcutaneous tissue and muscles) which are in communication with the lung through the afferent paths. These are the structures which take their innervation from the cervical portion of the cord. The reflexes through the vagus, on the other hand, manifest themselves in other organs that are supplied by the vagus, particularly the larynx, the heart and the gastro-intestinal tract.

The roentgenograph will show in shadow those areas in the lung which are sufficiently dense and of the proper chemical composition to produce a shadow. Inspection will show changes in the muscles, subcutaneous tissue and skin which result reflexly from the inflamma-

tion in the lung. It also shows us changes in the motility of the chest wall. Palpation offers us great service in confirming the changes in motility in the chest wall and also the motor and trophic changes in the soft tissues, and in determining the various changes which take place in the intrathoracic organs themselves (infiltration, emphysema, cavity formation, thickened pleura, etc.). Inflammation in the lung produces irritation of the sympathetic nerves which carry afferent impulses to the cervical portion of the cord. There these impulses disturb the equilibrium of other nearby nerve cells which give origin to motor and sensory nerves on the surface. As a result of this there is produced changes in sensation and increased motor activity (muscle spasm) when the disease is acute, and atrophy of the skin, subcutaneous tissue and muscles when the disease becomes chronic. Percussion as it has been taught has many sources of error, because we have had our minds centered too much on the infiltration in the lung, we have thought too much of the pathological anatomy accompanying the tubercle bacillus. The result obtained through the percussion stroke depends not only upon the infiltration in the lung, but upon all the tissues that are in the path of the stroke. If the muscles on one side are large and on the other side small, it can readily be seen that there would be considerable difference in the percussion of the two sides as a result of this. The same would be true if there is increased tone (muscle spasm) on one side and normal on the other, or if there is degeneration on one side and none on the other. The same is true of subcutaneous tissue,—if normal over one lung and degenerated over the other, this must be taken into consideration in the interpretation of the percussion blow. In intrathoracic diseases, there is a difference between such an inflammatory process as tuberculosis and a non-inflammatory process like carcinoma. The former produces more marked reflexes than the latter because the irritation to the nerves which supply the lung is greater. Consequently, their symptomatology differs. In a case of cancer, prior to the period of degeneration, there is not much coughing as a rule as long as the patient is quiescent,—in fact, there are not many of the symptoms that usually go with the inflammation of pulmonary tissue. The symptoms are due more to the mechanical influences of the growth.

It must further be remembered that auscultation and percussion were brought forth and developed almost to their present state before the modern intensive study of pathological conditions within the lung was made; before we knew the causes of tuberculosis; before the disease had been intensely studied, either pathologically or clinically.

Rales to my mind are among the most difficult sounds heard in the chest to interpret. The differentiation between rales of pulmonary and extra-pulmonary origin may be aided by a careful study of the

innervation of the lung and the extra-pulmonary tissues. The lung is supplied by the sympathetic nerves, which take their origin from the upper six thoracic segments and send the afferent impulses back to the cervical segments of the cord. The pleura is supplied by nerves which take their origin largely from the thoracic portion of the cord and send their afferent impulses back to the same segments of the cord. Consequently, the motor and trophic changes which occur in the soft tissues when the lungs are involved, and when the pleura is involved, express themselves in different areas of the body. Careful study of innervation and of the pathological changes resulting from inflammation in the different intrathoracic tissues will afford us great aid in interpreting rales.

Dr. DUNN: It has occurred to me that this discussion as to whether the X-ray can take the place of the physical methods of diagnosis is fooljsh. Our discussion years ago as to tuberculin resolved itself into the question of how much value was tuberculin, or any other method, in the treatment of tuberculosis. The question now seems to me to be, how much aid can the X-ray give us in diagnosis; not so much whether it is of more value, but can it help us to make better diagnoses, not only in regard to the cause of disease but as to the geographic distribution. I think that in diseases of the chest we must certainly look upon it as a material aid in regard to the geographic distribution of the lesions. In the doubtful or early cases it is of the greatest importance in ascertaining whether or not a patient has tuberculosis. As Dr. Miller has pointed out, it is of value in following up our cases; it is a permanent record; it is one we can study and go back to. The great danger, because of its ease and dramatic appeal, in lessening our dependence upon physical signs is very human. I would beg every man who is doing X-ray work on the chest not to look at the plate until he has followed all other methods, and then to resort to his X-ray plates and see how they check up with his other findings. He will find out from the X-ray plates certain things he has overlooked—he can go back and find physical changes he has overlooked. The X-ray does not necessarily have to destroy our skill as diagnosticians. On the contrary it should aid us to become better diagnosticians.

Dr. BRANNAN: I have found this discussion very illuminating. Recently I had a case in which I was in considerable doubt—a very early case that really ought not to have come to me. I sent the patient to Dr. Miller, he was away but his assistant returned me a diagnosis confirmed by the X-ray, which met all the indications in the case. Dr. Miller has said something about a physician's equipment not being complete until he can employ the X-ray himself. That would be an ideal situation, and I hope we

may all reach it. Of course all modern hospitals have good X-ray departments, and I am glad Dr. Miller has set the example of going over the plates himself and working out a diagnosis. I hear both sides. The medical men complain that they as a hospital trustee cannot get good plates or that they are not clearly interpreted. This applies particularly to the surgeons. They do not profess to know anything about the plates according to the radiographer, and yet when the rœntgenological diagnosis does not agree with theirs they are not satisfied.

Dr. DALAND: It seems to me that, as has been well said, the rœntgenological work may be looked upon as being an addition to the physical diagnosis, for the reason that the rœntgen ray is a physical agent and the results secured are physical, and therefore there should be no competition between the physical signs and the general history of the case on the one hand and the X-ray findings on the other. It seems to me furthermore that Dr. Miller has pointed out a very important fact; i. e., that a very large number of physicians as well as a large number of surgeons have been in the habit of demanding from the rœntgenological department a ready-made diagnosis; not so much that they may not know the physical signs but because they are lazy. I think this is the essence of the whole question. It is too easy—and then you have someone upon whom to put the blame, and if anything goes wrong the responsibility is evaded.

I believe that, as Dr. Miller has said, it is important that the physician as well as the surgeon should be in close touch with this work so far as a practical working out of the relationship of the internist to the X-ray is concerned. At the Charité a regular afternoon each week is devoted to a meeting of the staff, and all cases are gone over in consultation with the rœntgenologists and fluoroscopy is not neglected. The fluoroscop lends itself very readily to study on the part of internists, and the total amount of time required is not excessive. I think we should not under any circumstances abandon the study of the evidence that can be secured by experts in the making of these particular plates. This is the true situation, the advent of the X-ray opens up an enormously enlarged field for study in many directions. I do not think we should decrease one iota the advances that have been made by means of the X-ray. And on the other hand we should not decrease the value of all that has been obtained by other means of diagnosis.

One difficulty I have seen oftentimes is the apparent putting in disrepute of X-ray evidence which is purely physical because it does not make an etiological diagnosis. Our attitude should be one of complete coöperation and assistance. I find in my own work that the general tendency on the part of the physicians with whom I come

in contact is simply to accept the report of the rœntgenological laboratory as final. They oftentimes look with a very cursory gaze upon any evidence on the plate, and I know very few who will take the time to sit down and study these shadows. One made a statement regarding the geographic or topographic distribution of lesions within the lung. I think this is a very important matter, and I believe the here X-ray parallels somewhat the post-mortem study—namely, the amount of lesions determined by ordinary physical examination is probably only half that which you obtain from the X-ray examination or autopsy. Then too we are all struck by the convincing evidence given by the X-ray as to the existence of tumors, more particularly metastatic carcinomata. On more than one occasion I have been unable to satisfy myself that there existed a metastatic pulmonary cancer, and the plate has verified or darkened this suspicion.

In speaking of the relationship rather than the antagonism between physical signs and the X-ray we ought to carry in our minds one point very clearly, and that is, that the rœntgen ray has demonstrated the truth and falsity of many of the deductions hitherto drawn from the results of physical examination.

Dr. Minor: I have very little to say. I feel, after listening to those brief papers and to the discussions which followed, that the discussions may be perhaps more valuable than the papers themselves. I should like to thank Dr. Pfahler for the very broad way in which he talked from the X-ray man's point of view. He is a clinician as well as a rœntgenologist, and if all our rœntgenologists took his attitude this question would be nearer solution.

Dr. Miller very well stressed the extreme importance of combining in one man's hands if possible the two diagnoses. It is extremely important that this be attained. However there are not many men who will have the space, the finances and the time to put in such a complete photographic and stereoscopic apparatus. I fully recognize the limitations of the fluoroscope and the value of the photographic X-ray outfit, but I believe that the difficulty of operation and the expense of the latter point to the fluoroscope as the instrument for the general clinician. It isn't that the X-ray plate isn't a splendid addition; I wish that I could also have the advantage of such a stereoscopic outfit.

In regard to one small point, there is a difficulty that Dr. Manges has spoken of, that is, distinguishing between an interlobar pleurisy and the infiltrations that sometimes run out from the bronchial glands. The sharp lower border of the interlobar pleurisy and its change or disappearance or rotating the patient is the best means I know to distinguish the two, but I am sometimes put to it to be sure which condition exists.

I think Dr. Landis's historical point is extremely well taken, that the stethoscope was, when first introduced, looked upon in the same way that we are now viewing later additions to our knowledge, and that it is the natural tendency of human nature to run to the latest thing and over stress the value of the latest thing.

Dr. MANGES: Unfortunately to save time I cut out the first few pages of my paper in reference to bronchial pneumonia. My remarks are based entirely on conditions in adults, until the bronchopneumonic area is sufficiently large it is of relatively little value in adults. In regard to the question of lesions on the left side I referred to a type of pneumonia which had a very large diaphragmatic inflammation.

In regard to the stand taken by Dr. Pfahler, who speaks as one of us, I can only say that if every rœntgenologist took his view there would be a common ground, and medicine would be the gainer and rœntgenology would be the gainer. The trouble is, medical men and surgeons are hurting rœntgenology. I heard Dr. Tuffier say that in his surgical work he depended absolutely on rœntgenology. This is hurting the X-ray. There is a great future for the X-ray. There is a new surgery in the field—the surgery of the lung—that is going to improve greatly many of those cases that have baffled medical skill. It is here where the X-ray has an enormous value. It is a big field, but a field in which the X-ray is only a part. In the future work on the surgery of the chest and lung there is a path that is going to be blazed by the X-ray.

Dr. PRATT: I have just a few words to say. Dr. Pottenger thinks that the orthodiagraph is not necessary if we make telerœntgenograms. I tried to show that the slight changes which are of clinical significance in the heart concern the form, the shape and the size. These slight changes in the shape cannot be determined unless we measure accurately by the dynameters (?) that von Moritz has given us, the length, the breadth and the horizontal diameter. The trouble with the telerœntgenograph is that in most cases the plates do not show these points clearly, especially at the apex; and yet unless you determine the apex you cannot determine the length, and furthermore you cannot determine the breadth. This is of importance, because, as I tried to point out, an increase in breadth indicates an enlargement of the right heart. This cannot be determined from the rœntgenograph and hence we must wait for the orthodiagraphic tracing. Why is it that the orthodiagraphs have been thrown out on the dump heap? I am sure it is because clinicians have not thoroughly studied the matter. But they are of great value to clinicians. I think, as has been said, that there should be a correlation between these two fields and that more and more clinicians should do this work themselves.

Dr. Pfahler: I will be very brief. Any phase of this subject might be discussed for the rest of the day, so that it is really not worth while taking it up. The whole discussion this morning seems to have centered around a general praise for X-ray work, or rœntgenology, and so the few minutes I have I will use for discussion of that phase of the subject. There was a time when every practitioner did his own surgery, his own urinalysis, his own pathological work and everything that was to be done, and we know how well he did it. Now when every man begins to do his own rœntgenology, I say if you are able to do it well do it, but don't substitute poor work for good. We can't all be good clinicians and give the time that is necessary to gain a livelihood by the practice of medicine, and become experts in rœntgenology. It is a physical impossibility. I speak knowingly. I associate with men who have spent four, five and six years, spending all day, at the work of rœntgenology and studying it in connection with clinicians, and I see how imperfect their work is. And if those men, spending their entire time, still lack the skill they ought to have, what are you going to learn when you pick up a few cases and study them? I say again that if you can do this work well do it, but don't substitute poor work for good work. There are men making X-ray examinations who are not fitted to do it. I have had patients sent to me who say: "Why, Doctor, I had an X-ray examination done by Dr. So-and-So." "What did he find?" "So and so." An X-ray examination made by such and such a man, who had not had sufficient experience to give an expert opinion, and so gave one that was entirely wrong. Don't get an idea that by buying a machine you can become a rœntgenologist. Dr. Minor has become a rœntgenologist familiar with the fluoroscope. He has been at it nineteen years, and he has worked in one line—tuberculosis. Of course he has a lot of skill. If you can become sufficiently skilled in this work, why do it, but don't try to blind yourselves by the thought that you are giving valuable evidence because you are doing the work yourselves. It is a method of physical diagnosis which requires a great deal of expense, a great deal of assistance and a great deal of time—and you don't have enough time. Not that you haven't the brains—but you haven't the time, space and money to put into it. The rœntgenologists have been encouraged by men who take their evidence too seriously. You have to keep in mind the man who is giving the evidence. That applies particularly to surgery, to physical diagnosis, to the expert in tuberculosis. For instance, I know Dr. Daland and Dr. Landis, and if they give me an opinion on a tuberculosis case it is settled, but just because somebody gives me an opinion, somebody I don't know anything about, I am not willing to accept it; until I know the man I don't approve it—there is the personal factor back of it.

Dr. BRANNAN: It must have been seven or eight years age, at the time of our International Congress of Tuberculosis, that Dr. Rey (?), of Paris when visiting Bellevue, expressed surprise that American physicians did not use the fluoroscope, and said that in Paris they used it as a part of their routine examination in all cases of internal medicine.

A STUDY OF VASOMOTOR EFFICIENCY.

By GEORGE W. NORRIS, M.D., and FRANKLIN F. LANE, M.D.

That the functional efficiency of the circulation bears a definite relation, in direct proportion, to the health of the individual has been demonstrated by many experimenters, but just which diseases cause the greatest loss in efficiency and which recover the soonest, has not been dwelt upon to any great extent.

With this in view, at the suggestion of Dr. George W. Norris (who is now "somewhere in Europe," and whom the author was to assist in writing this paper by gathering the data), blood pressure readings were made on a series of cases in the Pennsylvania Hospital.

There have been several methods devised by different authors to measure the vasomotor efficiency; some complicated and others fairly simple in their technique. Most of these methods are based on sound theoretic principles, but they are not absolutely accurate, due to the numerous sources of error. They are, however, sufficiently accurate to form a basis upon which to work. One great source of error is the personal equation. This may be cut down to a minimum, however, if, in a long series of cases, one person makes all the readings.

The value of any test for the functional efficiency of the circulation is three fold. First, from the standpoint of diagnosis; secondly, from the standpoint of prognosis; and, thirdly, from the standpoint of the efficacy of the therapeusis.

The cases for experiment were divided into three classes as follows:—

Class A: Those with cardio-vascular diseases.

Class B: Those with some constitutional disorder.

Class C: Those in comparative health. As all of this class

were hospital cases, few individuals in really good health could be obtained. The results from this class cannot be of as much value as the first two, except for comparison.

This group, however, is the most interesting from the three standpoints of diagnosis, prognosis and therapeutics. In the first two classes there are many methods of diagnosis, without a test for vasomotor efficiency. The prognosis and the value of the treatment of a case may be told by the disappearance of œdema, dyspnea, cyanosis and other pathological findings and the reappearance of normal conditions.

In class *C*, however, come the cases which, though apparently well, yet lack the stamina and punch to carry through a day's work, and those who are always complaining, yet in whom no pathological condition can be found. There is something wrong somewhere, but the finger cannot be placed on it.

These cases are usually of two types, both having low blood pressure; latent syphilis and undemonstrable tuberculosis.

While a definite diagnosis cannot be made by blood pressure and pulse readings, yet by the low blood pressure and low percentage of efficiency of the circulation, the underlying causes of the continued weakness can be recognized, the proper treatment instituted and the prognosis noted.

An overtrained athlete can easily be detected by the thin pinched face, the dry shiny skin, and the haggard look about the eyes. But one who is just on the verge of overtraining and just beginning to go backward is difficult to recognize.

A very good idea of the physical fitness of the athlete, or non-athlete, can be easily determined by a test for the vasomotor efficiency. This test is simple and easily carried out by anyone. It does not require any special knowledge or training. A blood pressure apparatus, a watch, and a scale on which to grade the findings are the only necessary implements, and it takes less than five minutes to perform the test and calculate the result.

The Crampton test was selected as the most desirable on which to form a basis for these experiments.

This test, as do the others, depends on the manner in which the pulse rate and blood pressure respond—quantitatively, qualitatively and temporarily—to definite changes in posture. In rising from the recumbent to the erect posture blood pressure tends to fall as the result of gravity. In health this tendency is automatically regulated. In a healthy, vigorous man this change in posture gives a rise of blood pressure amounting to 8 or 10 mm. of mercury and an increase of pulse rate from 0 to 4 per minute. This increase may result from increased vasomotor tone, from increased cardiac effort, or from both. With these facts in mind, Crampton devised the following table to test vasomotor efficiency by observation of the pulse rate and blood pressure responses to a change of posture.

Vasomotor Tone Changes.

1. In health, the vasomotor tone is increased and the blood pressure raised 10 mm. Hg on changing from the recumbent to the upright position.

Percentage Scale.

(Vasomotor Tone.)

Blood Pressure.

% = % of efficiency of vasomotor system.

Heart Rate Increase	Increase Blood Pressure					No Change Blood Pressure	Decrease Blood Pressure				
	+10	+8	+6	+4	+2		−2	−4	−6	−8	−10
0–4 . .	100%	95%	90%	85%	80%	75%	70%	65%	60%	55%	50%
5–8 . .	95	90	85	80	75	70	65	60	55	50	45
9–12 . .	90	85	80	75	70	65	60	55	50	45	40
13–16 . .	85	80	75	70	65	60	55	50	45	40	35
17–20 . .	80	75	70	65	60	55	50	45	40	35	30
21–24 . .	75	70	65	60	55	50	45	40	35	30	25
25–28 . .	70	65	60	55	50	45	40	35	30	25	20
29–32 . .	65	60	55	50	45	40	35	30	25	20	15
33–36 . .	60	55	50	45	40	35	30	25	20	15	10
37–40 . .	55	50	45	40	35	30	25	20	15	10	5
41–44 . .	50	45	40	35	30	25	20	15	10	5	0

Note: In case of increase in pressure higher than + 10 add 5 per cent. to the + 10 column for each rise of 2 mm. Hg in excess of 10.

In disease (mental or physical), blood pressure either remains the same or falls more than 10 mm. Hg. It may be considered normal to fall 10 mm. Hg.

2. Heart rate. In health, no change on change of position.

In disease, increase to as much as 44 beats per minute.

This change in pulse rate may take the place of change in blood pressure.

Decrease of 1 mm. Hg = increase 2 pulse beats.

The readings are made with the patient in the recumbent posture, by counting the pulse until two successive quarter minutes are of the same rate. The rate for one quarter is then multiplied by four and recorded. The systolic blood pressure is then taken by the auscultatory method. The patient then stands and when the pulse has steadied and two successive quarter minutes are alike the rate per minute is recorded, the systolic pressure is again taken and recorded. By comparing these results with the Crampton scale a numerical statement of the function may be obtained.

Class *A*.

Ten cases were studied, all with signs of decompensation. When compensation had been established for three or four weeks they were again studied. After eight weeks there were four remaining in the hospital due to complications such as anæmia, aneurysm or nephritis, and these cases were again recorded.

Before compensation the average vasomotor efficiency according to the Crampton scale was 62.5%
After compensation had been established for three or four weeks the average was 93.6%
After four weeks more of treatment the four remaining averaged 97.0%
The lowest of the ten before compensation was 35.0%
The highest was 75.0%
After compensation was established the lowest was 80.0%
And the highest was 100.0%

Class *B*.

Fifty-eight cases in all were studied. This group consisted of the following convalescent patients:

Diseases	Number of Cases
Pneumonia	10
Acute rheumatic fever	5
Acute bronchitis	5
Gastric ulcer	5
Acute plumbism	4
Typhoid fever	3
Chronic nephritis	3
Diseases of thyroid	3
Acute appendicitis, post-operative	3
Acute tonsillitis	2
Empyæma	2
Tuberculosis of kidney and bladder	2
Sapræmia	1
Lung abscess	1
Perianal abscess	1
Necrosis of jaw	1
Cervical adenitis	1
Sarcoma of wrist	1
Diabetes mellitus	1
Mucous colitis	1
Myasthenia gravis	1

All of the above cases were not studied until they were ambulatory and had had from one to three months' treatment.

This series averaged	45.8%
The lowest was	10.0%
The highest was	67.0%
After one to four weeks' additional treatment those remaining (about 40) averaged	81.0%
The lowest being	40.0%
The highest being	100.0%

Class *C*.

As stated before, none of these patients could be classed accurately as healthy, vigorous subjects except in a comparative way. They were all apparently strong.

In all, seventeen cases were studied, consisting of the following:

Diagnosis	Number of Cases
Fractures	6
Hernia	3
No pathological condition	3
Conjunctivitis	2
Rheumatoid arthritis	2
Old infected hand	1
These averaged	79.3%
The lowest was	55.0%
The highest was	100.0%

Summary.

Class *A* (cardio-vascular):	
Before compensation	62.5%
After compensation	93.6%
After eight weeks of compensation	97.0%
Class *B* (convalescent infections, 4–12 weeks in bed):	
As soon as out of bed	45.8%
After 1–4 weeks more treatment	81.0%
Class *C* (in apparent general physical health):	
Only one reading for each patient	79.3%

Conclusions.

1. Cardio-vascular conditions do not seem to affect vasomotor efficiency as much as do the prolonged infections and other constitutional disorders.

2. That the latter group of diseases affect the system more profoundly is shown by the length of time to recover their vasomotor tone.

3. After compensation has been established vasomotor efficiency is surprisingly good, while after an acute infection it is surprisingly low, although the individual seems to be in excellent physical condition.

4. Although class *C* is composed of comparatively healthy individuals yet the results indicate that their vasomotor efficiency is higher or lower in proportion to their general health. Taken as a whole, the findings of this group were higher than either of the other two, these being a nearer approach to the normal. This would seem to indicate the value of the Crampton test in:

(*a*) Diagnosis of physical fitness.

(*b*) Furthermore, it seems to have some prognostic value, inasmuch as readings taken at different periods seem to indicate whether the individual is returning to normal or not.

(*c*) Finally, the method furnishes a clue as to the efficiency of the therapeutic measures employed.

DISCUSSION.

Dr. MINOR: In looking over Dr. Bracken's most interesting questionnaire it becomes evident how many more questions we could add. Indeed to discuss these matters thoroughly and satisfactorily would take us weeks. The questionnaire is an excellent one. One of the most important questions of course is that of childhood infection. The work of von Pirquet, Naegeli, Norris and others has shown the frequency of childhood infection, and unquestionably as a result a great many men are teaching perhaps too enthusiastically the fact that infection after sixteen or eighteen is a relatively unimportant matter. No one here would deny that any living person can become infected for the first time with tuberculosis. From statistics, those of Naegeli for instance, we must recognize, if he is correct, that no one after the age of thirty can become infected for the first time. We do not know that previous infection in every case provides immunity. We know that a scrofulous child has a certain immunity against tuberculosis, but we can't say that John Smith, who has become infected at six or eight, is immune to a reinfection with tuberculosis in later life. I myself would under no circumstances wish to live in a crowded, dirty place with a more or less consumptive person—and I have already had the disease. I am not at all sure that I might not get a superimposed infection. But when it comes down to the broad question I do believe that it is right to tell people that the chief danger is in childhood and to warn them in regard to the care of their children, always taking for granted that the patient is so sensible that you can be sure he is absolutely careful. I am convinced that there is not one man here who is not sure he could live in a house with a consumptive if the patient was absolutely clean and absolutely careful; but slips come in everywhere, and therefore I warn especially against childhood infection.

Another of Dr. Bracken's questions comes very close to me as it criticises The Handbook of the Nat. Association for which I am personally responsible. I am sorry he did not read further in the article from which he quoted before he criticised. The paper reads: "How can you and your family prevent others from

getting the disease? First, by formaldehyde disinfection." We are all familiar with the limitations of formaldehyde disinfection; but this is to go out to poor people who cannot afford expensive methods and therefore I gave the sheet method as about the best they could do. "Second"—and this is in heavy lettering—"*Far more important is hard scrubbing followed by fresh air and sunlight. Remember that mere fumigation with formaldehyde and the letting in of fresh air and sunlight will not kill the germs of tuberculosis. Therefore*"—Dr. Bracken—"*after the formaldehyde disinfection wipe down all the ceilings and walls with some disinfectant solution, then scrub with water and carbolic acid, and open all windows to the sunlight for two days—then you need have no fear of infection in that room.*" Of course Dr. Bracken may not understand it, but the people to whom it is to go will understand because it is put in large black letters and underscored. I thought I had protected myself against misunderstanding, but if Dr. Bracken misunderstands I am afraid I will have to get him to rewrite the article for me so that it may be plain.

Dr. POTTENGER: First, how do children become infected? Those of us who have followed the work of some of the German writers know that they have taken children who lived in country districts where there were no tuberculous families, and yet those children still showed a percentage of 30 to 45 infections before school life. Another factor is that these children do not live on cow's milk, but nearly all are nursed by their mothers until two years of age; so that it does seem that we have got to find some other means by which the tubercle bacillus is carried.

Regarding children becoming infected and whether we should be careful after they are infected, of course the question is one of dosage. If you watch children you will find that those who are associated most intimately with a tuberculous individual are most apt to break down clinically. I think a child should be removed if possible from a tuberculous environment even if it is already infected.

Dr. HAWES: I should like to say a few words in regard to Question 6, Should every case of tuberculosis be reported? In Massachusetts tuberculosis is classed among diseases dangerous to public health and must be reported. I frequently do not report cases of tuberculosis. If I did I should have to report a patient coming back from Saranac in perfectly good condition. I do not report a case until I am pretty sure it is tuberculosis, and I keep many patients under observation for a long time. Those of you who are doing consultation work I think will agree with me. If a man sends a patient to me am I to report the case over his head? I should make myself extremely unpopular if I did. I don't report every case of tubercu-

losis, and until the word tuberculosis is defined I do not think we can do so. If it were said, "every case of pulmonary disease with bacilli in the sputum" it would be a different matter.

In regard to Question 11—What should be done with the incorrigible tuberculous? A bill in regard to this comes up every year before the legislature of Massachusetts. This year it went as far as the third reading in the House and then was killed. I don't think such a bill will ever be passed. Public opinion is against it, and in Massachusetts you can't go ahead of public opinion.

Dr. PERKINS: Question 8 I think is important. Has the sanatorium a place in the control of tuberculosis? One factor which leads one to question some of the claims made as to the value of the sanatorium is the patient coming back from the sanatorium with the disease more or less arrested but bacilli still in the sputum. That individual has had his life lengthened by going to the sanatorium He has been put in a better physical condition; but if he is a poor man he comes back and lives under poor conditions and is going to spread the disease and a little later die of it.

In regard to the incorrigible consumptive we have had a little better success than in Massachusetts. We tried to get a pretty vigorous law through this year. We didn't get it through but we got one through which allows the health officer to consider an incorrigible consumptive just as he would consider any other case of contagious disease; and we have had none sent to the state institution under that law as yet, but we have had them sent to other institutions.

I am very much interested in the question of the preventorium. We have a preventorium which I think I was responsible for starting and which I believe was the first one started after the one here in New Jersey. I believe the preventorium is a most efficient method of helping to control tuberculosis, by taking the children from infected families, children who show some signs of the disease—not necessarily anything you can put your hand on as being the direct effect of infection, but yet who do show the signs so well known and demonstrated by children infected with tuberculosis—and by taking them to the preventorium and building them up and keeping them for quite a length of time we have been able to put them in good physical condition. We have been in operation long enough to follow half of them at least for quite a long time, and where they have been able to go back to good home environments they have remained in good condition. Personally I believe that this is one of our best methods for fighting tuberculosis—to take these children whom we have every reason to believe are infected and build them up to make them resistant against further infection.

Dr. ELLIOTT: What is a preventorium? I have been a director of the Preventorium at Toronto since its inception. There we try to prevent the non-reactor from becoming a reactor, and the child who is a reactor from becoming clinically tuberculous. In our clinic at the Children's Hospital at Toronto we have children referred from various sources. The child who has clinical tuberculosis is sent if possible to a sanatorium for tuberculous children, not to an adult institution. The child who is a reactor or a contact case where there is open tuberculosis, is sent if possible to the preventorium during the presence of that open case in the house. The child who is a reactor, who is underfed, undernourished and whose home conditions are good is for the open-air school. He is fed at the expense of the school department, and goes back to his home at night—where the sanitary conditions are kept up by the Health Department. If the home conditions are not good he is sent to the preventorium and is kept there until such time as the home conditions are made right or the open case is removed.

Dr. BROWNING: I want to say just a word in regard to Dr. Bracken's inquiry as to whether or not sedatives should be used. I should perhaps modify my reply and confine it especially to the preparations of opium, because that is the class of remedies I find most frequently used. I believe that the excuse for the use of opium in the tuberculous case is extremely rare, except in cases in extremis; but we find it frequently being used for cough and other complications, more especially for cough and to procure rest at night. The objection to it is that even in small doses the habit with many persons becomes formed, and the patients who take opiates certainly lose their will power and will not make an effort to prevent cough. Again, it changes their dispositions very materially. When I took the tuberculosis service at the County Hospital at Los Angeles some four or five years ago it was the custom to give to patients who coughed at night some opiate, usually in very small doses, the excuse being that it was impossible for the other patients to procure rest The patients were grouchy and there were fights on the wards so that they had to call in the sheriff. They complained of the food; they were abusive to the nurses, and there was a great deal of trouble, with a new interne coming on at night, to control them. Finally we have cut off all opiates from those wards; and only under very extreme circumstances—not even as a routine measure or infrequently in cases of hæmorrhage—are they permitted, and the patients have gone on in a way that is really most gratifying. There may sometimes be excuses for some of the other sedatives, but I believe that opiates should be eliminated almost entirely from the treatment of tuberculous patients, except possibly in extremis and very, very rarely in hæmorrhage.

Dr. BRACKEN: I am very glad to make my apology to Dr. Minor. I want him to understand that I am thoroughly in sympathy with his pamphlet. On page 11 of the pamphlet is the reference to formaldehyde disinfection—I believe that this is the best disinfectant we have; but the fact is that the only method of formaldehyde disinfection given is the sheet method, which is now admitted to be of little value. Dr. Minor spoke of recommending this method to people who are poor—there I want to bring out another point. Disinfection of the premises is not for the benefit of the people who have been living in them. Disinfection is a community problem, not an individual problem, and should be done only by trained disinfectors. My apology to Dr. Minor is that I did not have time to cover the subject.

The next point, the reporting of cases of tuberculosis, is rather interesting.

Dr. Elliott gave my idea of a preventorium—taking the children, as he says, from bad family surroundings and giving them a chance to get well, or keeping them from developing the disease. But it is not my idea that anæmic children, run down physically but not in contact with tuberculous cases, should be sent to a preventorium. The place for these children is the open-air school, where they can get the proper feeding, the proper rest and the proper supervision. That is my idea of the difference between an open-air school and a preventorium.

I used the term sedatives, but of course in using it I meant only opium and its derivatives.

TOPICS

For Consideration by the American Climatological and Clinical Association.

Presented by Dr. H. M. Bracken, St. Paul.

1. Can the danger from an open case of tuberculosis be considered negligible so long as there are no small children associated in residence with said patient?

2. Is it to be admitted that the infection of adults with tuberculosis can be considered a negligible quantity?

3. Given the case of a babe that has been living with a tuberculous mother under conditions permitting possible infection of child before the condition of the mother is known: What course should we follow with the child? Shall we assume that it is already infected from the mother and that therefore no action need be taken looking to its possible future infection from the mother?

4. How do so many children become infected with tuberculosis in infancy, when but a very small percentage come in contact with open cases of tuberculosis?

5. Is tuberculosis a public health problem?

6. Should every case of tuberculosis be reported?

7. Should there be sanitary supervision of every case of tuberculosis?

8. Has the sanatorium a place in the control of tuberculosis?

9. Should all tuberculous patients be sent to an institution (sanatorium or hospital) for the tuberculous?

10. Should all open cases of tuberculosis be compelled to stay in an institution (sanatorium or hospital) for the tuberculous?

11. What should be done with the incorrigible tuberculous?

12. Why disinfection after tuberculosis?

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